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Are Minimum Wage Effects Greater in Low-Wage Areas?

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Abstract

Empirical work on the minimum wage typically estimate effects averaged across high and low wage areas. Low wage labor markets could potentially be less able to absorb minimum wage increases, in turn leading to more negative employment effects. In this paper we examine minimum wage effects in low wage counties, where relative minimum wage ratios reach as high as .82, well beyond the state-based ratios in extant studies. Using data from the ACS, the QWI and the QCEW, we implement event study and difference-in-difference methods, estimating average causal effects for all events in our sample and separately for areas with lower and higher impacts. We find positive wage effects, especially in high impact counties, but do not detect adverse effects on employment, weekly hours or annual weeks worked. We do not find negative employment effects among women, blacks and/or Hispanics. In high impact counties, we find substantial declines in household and child poverty. These results inform policy debates about providing exemptions to a $15 federal minimum wage in low-wage areas.

JEL Classification: J20, J31, J48, J80

Keywords: minimum wage, employment, median wage, low-wage areas, poverty

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1. Introduction

This paper examines the effects of federal and state minimum wage increases in low-wage counties. While a majority of empirical work fails to find significant disemployment effects of the minimum wage, these studies typically estimate an aggregate employment effect, averaging effects for high and low wage areas. In low-wage counties, where the fraction of workers employed in jobs paying close to the minimum is relatively high, minimum wage increases may be more effective in raising average earnings. At the same time, low wage labor markets could potentially be less able to absorb minimum wage increases, in turn leading to more negative employment effects. In this paper, we examine these effects using sub-state data from across the United States, permitting us to observe effects in areas where exposure to minimum wage work is significantly higher than has been studied in previous work using state-level data.

More specifically, we study the effects of high relative minimum wages and high minimum wage bites at the county level. We construct two well-established measures of local exposure to the minimum wage: a) the *relative* level of the minimum wage—defined as the ratio of the minimum wage to the median wage; and b) the *bite* of the minimum wage—defined as the proportion of workers who receive a pay increase if the minimum wage increases. While each of these measures provides an indicator of the intensity of the policy, the relative minimum wage and the bite are more sensitive to labor market conditions in lower-wage areas.

Research on recent state-level minimum wage policies does not currently extend beyond the $10 level; the highest studied state-level relative minimum wage is .59 (Cengiz et al. 2019). Studies of local minimum wages extend higher — as much as $13 in 2016 (Allegretto et al. 2018). But since local areas with high minimum wages also tend to have relatively high median wages, their relative minimum wages and bites are close to the U.S. average.

Sub-state variation in wages has been under-utilized in recent minimum wage research. In every state, counties vary considerably in their median wages. As a result, the ratio of minimum wages to county-level median wages varies much more than do the state-level ratios, with much higher ratios in lower-wage areas. Many, but not all, of the high relative minimum wage counties are in the 21 states that have remained at the federal minimum of $7.25 since 2009; yet evidence from these counties has not played a role in recent studies. Moreover, much of the concern about a $15 federal minimum wage concerns the lowest-wage states. County-level variation in these states and others thus provides an important opportunity for studying the effects of high minimum wages in low-wage areas.

We use data from the American Community Survey (ACS) for our main analysis. ACS data are available beginning in 2005. The large sample size of the ACS allows to analyze data at a more fine-grained geographical level. The ACS directly identifies only the more populous counties, covering about 60 percent of the U.S. population. To be able to include data on all counties, including those in rural areas, we also use local areas based on census-defined Public Use Microdata Areas (Pumas) — areas of about 100,000 people. As a check on our results, we implement a similar approach using county-level data on employment and earnings in the Quarterly Workforce Indicators (QWI) and the Quarterly Census on Employment and Wages (QCEW). While the ACS is based on survey responses by households, the QWI and QCEW are based on administrative data submitted by employers.

Our analysis leverages variation in state minimum wages over time to estimate event study and generalized difference-in-difference models. We examine wage, employment and poverty outcomes in samples of those who are most exposed to minimum wages: those with a high school education or less, teens and workers in food service and retail—the two lowest-wage industries. We report average results for all the areas in our sample, and separately for those with higher relative minimum wages or higher bites. To check that our methods identify causal effects, we conduct tests for common pre-trends as well as robustness and placebo tests.

Our results generally suggest the presence of positive wage effects. We show that these wage effects are greater in areas with higher relative minimum wages and bites, validating our approach to studying high impact areas. We do not detect adverse effects on employment, on either the extensive margin (working at any time during the reference year), or on hours or weeks worked. We also do not find negative employment effects among blacks, Hispanics and women. We do find reduced household and child poverty in counties with high relative minimum wages, up to .82, and as well in areas with especially high bites.

We analyze two additional channels of adjustment. First, higher minimum wages may force workers living in low wage areas to accept jobs further away from home, leading to an increase in commuting. Second, workers could adjust to reduced labor demand by shifting into non-standard work arrangements, leading to an uptick in independent contracting. Our models fail to find evidence supporting either of these two hypotheses: out-of-area commuting does not shift with the minimum wage, and we do not detect a reallocation to independent contracting.

The minimum wage is one of the most studied topics in economics. Under perfect competition, economic theory predicts that a higher binding minimum wage will lead to job loss, as some workers are priced out. If perfect competition fails, e.g. due to the presence of search frictions or monopsony power, predicted employment effects are ambiguous. A large number of published empirical studies have analyzed employment effects empirically, with sometimes conflicting results. In a comprehensive analysis, Cengiz, Dube, Lindner and Zipperer (2019) examine the effects on jobs of 138 prominent state minimum wage events between 1984 and 2016. The authors do not detect significant negative effects on the number of low-wage jobs.[1] Their results are consistent with a meta-analysis of minimum wage studies by Belman and Wolfson (2019). Other studies, such as Clemens and Wither (2019) and Meer and West (2016), find negative employment effects.

In a recent review of the literature, Dube (2019) proposes a consensus view: Minimum wage increases have had modest to minimal negative employment effects. However, the studies reviewed by Dube examine policies that raised the relative minimum wage to no higher than .59. Negative employment effects, especially those due to automation or competition from other areas for tradeable goods, may be greater at higher minimum wages.

Recent policy discussions have brought the possible effects of higher minimum wages to the fore. For example, in 2019 the U.S. House of Representatives passed a bill to phase in a federal $15 minimum wage over six years. This bill would increase the relative minimum wage to about .67 nationally, and to about 0.8 in the lowest-wage states, such as Alabama or Mississippi (Reich 2019). It is therefore important not just to study average minimum wage effects, but also to consider heterogeneous effects, especially in high-impact areas.[2] Our study focuses on effects of minimum wages across counties and other small areas with different relative minimum wages,

[1] Cengiz et al. conduct numerous stress tests of their findings, including possible lags and leads, effects by subsample period, placebo tests, robustness to including possible confounding variables, effects on individual demographic groups, and tests of substitution of educated workers for less-educated workers.
[2] A December 30, 2019 New York Times editorial, "Double the Federal Minimum Wage," asserts that an exemption should be provided for the lowest-wage areas. https://www.nytimes.com/2019/12/30/opinion/federal-minimum-wage.html

using the wider variation in relative minimum wages that exists between localities within each state. Thus we are able to observe minimum wage effects where the Kaitz ratio goes as high as .82, thirty percent higher than in any previous minimum wage study, yet near the range of recent minimum wage policy proposals.

We are not the first to use county-level variation to study minimum wage employment effects (see, e.g., Thompson 2009, Dube et al 2010, Addison et al 2012, Dube et al 2016). Thompson (2009) uses county-level data to identify groups of high and low impact counties. Thompson's empirical analysis exploits the greater variation across counties to identify impacts of change on teen employment by comparing changes in employment in high and low impact groups following a federal minimum wage increase. His paper thus expands upon the approach of Card (1992), in estimating a difference in difference model where differential changes employment rates in high impact counties after the policy change are attributed to the minimum wage. Like Thompson, our paper uses county-level wages to assign counties to high and low impact samples. However, our empirical models are fundamentally different: our samples include both federal and state minimum wage changes, and we obtain identification through the differential timing of these policy shifts.

Conversely, Dube et al. (2010) analyze effects of minimum wage differentials within border-county pairs, generalizing the case-study approach of Card and Krueger (1994). Their analysis leverages policy discontinuities at state borders, allowing the authors to control flexibly for spatial heterogeneity in wages and employment. Although their analysis relies on state-level variation in minimum wages, their analysis differs from ours. First, their models focuses on contiguous counties, while our analyses includes the full sample of localities. Relative to the border county pair framework, our models thus make stronger assumptions regarding spatial heterogeneity in underlying wage and employment trends. Second, like the large majority of minimum wage research, Dube et al. estimate average effects across localities, while our paper focuses on heterogeneous effects in areas with high and low exposure rates.

Our analysis follows Zipperer (2014) in using the local relative minimum wage/bite as proxies for the expected impact of minimum wage changes on local wage levels. We use two well-established measures of exposure: the relative minimum wage (Kaitz index) and the bite (share of workers paid less than the minimum wage). Both metrics have been widely used in the minimum wage literature, but in different ways.

The Kaitz index has historically been used in empirical work on minimum wages as a parametrization of minimum wage policy intended to capture the "effective" minimum wage in the presence of heterogeneous wage and price levels. However, Card, Katz and Krueger (1993) showed that relative minimum wages vary more with the median wage than with the minimum wage, confounding whether the relative minimum wage measures policy variation. If unobserved shocks to the economy shift both median wages and employment rates in the same direction, relative minimum wages will be negatively correlated with employment rates even when there is no variation in actual minimum wage policy. This critique led minimum wage researchers to drop the use of the relative minimum wage in statistical analysis.

Similarly, researchers sometimes use variation in the bite of the minimum wage to identify effects of minimum wages in the absence of state-level variation in policy, e.g. to analyze impacts of federal minimum wage legislation (Card 1992, Thompson 2009, Bailey, DiNardo and Stuart 2019). In Appendix B, we discuss these methods in more detail, together with a discussion of how the results from these methods differ from our preferred specifications.

The rest of the paper is structured as follows. Section 2 discusses our research design, including our data and empirical methods and descriptive statistics. We present our results in Section 3 and then summarize and conclude in Section 4.

2. Research design

Our research design focuses on effects of minimum wages across counties and other small areas with different relative minimum wages, using the wider variation in relative minimum wages that exists between localities within each state. We are not the first to use county-level variation to study minimum wage employment effects (see Card 1992) or to use the relative minimum wage metric. However, Card, Katz and Krueger (1993) showed that relative minimum wages vary more with the median wage than with the minimum wage, confounding whether the relative minimum wage measures policy variation. If unobserved shocks to the economy shift both median wages and employment rates in the same direction, relative minimum wages will be negatively correlated with employment rates even when there is no variation in actual minimum wage policy. This critique led minimum wage researchers to drop the use of the relative minimum wage in statistical analysis.

Similarly, researchers sometimes use variation in the bite of the minimum wage to identify effects of minimum wages in the absence of state-level variation in policy (Card 1992, Bailey, DiNardo and Stuart 2019). In Appendix B, we discuss these methods in more detail, together with a discussion of how the results from these methods differ from our preferred specifications.

In this paper, we do not use the relative minimum wage as a measure of minimum wage policy. Rather, we follow Zipperer (2014) in using the local relative minimum wage as a proxy for the expected impact of minimum wage changes on local wage levels. We then estimate a set of event study and generalized difference-in-difference regressions, estimating effects of the minimum wage on wages and employment in high and low impact regions.

In the following, we first present the data used for the analysis. Then, we present the empirical models, followed by descriptive statistics on the geography and characteristics of high impact areas.

2.1 Data

Our main data source is the 1-year estimates from the American Community Survey (ACS), which is available for the years 2005 through 2017. The primary advantage of the ACS for our purposes is its large sample size – the ACS samples approximately 3 million addresses a year, compared to around 100,000 for the Current Population Survey – as well as its much higher response rate. The larger sample size allows us to credibly estimate local median wages as well as wages and employment rates for various demographic groups for smaller localities by calendar year.

The 1-year ACS files directly identify only a subset of counties; the identity of counties with a population below 65,000 is suppressed. In addition, we do not observe counties whose borders do not line up with those of the census-designated public use microdata areas (PUMAs).[3]

[3] PUMAs consist of areas with at least 100,000 residents. The ACS provides PUMA information on all respondents. In less-populated areas, PUMAs typically consist of two adjacent counties. In more-populated areas, counties contain multiple PUMAs. Los Angeles County, for example, has over 30 PUMAs. In such areas, workers' relevant labor markets are better defined by their county than by their PUMA.

As a result, only about 60 percent of the U.S. population resides in counties that are directly identified in the ACS. To overcome this problem, our empirical analysis instead uses “coumas” -- geographic areas defined by Case and Deaton (2017) in their work on deaths of despair. For every county and consistent PUMA, a couma corresponds to whichever has the *larger* population -- the county or the PUMA.[4] The larger unit better captures the relevant labor market. Coumas then cover the entire U. S. population, including rural as well as urban areas. In 2017, there were 708 coumas; the median couma had 223,133 inhabitants.

The ACS contains a rich set of background variables as well as information on employment and earnings. For our key variable of interest -- the hourly wage – the ACS contains two disadvantages relative to the CPS. First, data on hourly earnings are not reported directly in the survey, but must be estimated by dividing the previous year’s annual earnings by the product of weekly hours worked and yearly weeks worked. Each of these steps introduces measurement error, especially for part-year workers, as the number of weeks worked is reported in bins rather than as an exact number. This data issue adds noise to the hourly earnings variable, but not bias. Second, since respondents are surveyed throughout the year, the reference period varies by the month of the survey. To keep the analysis tractable, all responses are assigned the same reference period (the calendar year before the survey).[5] Our use of the ACS to study minimum wage effects is supported by the example of Clemens and Strain (2018), who report that they obtain similar results with the ACS and with the CPS.

We identify several groups of workers that might have high exposure to minimum wage work. In our most expansive definition, we include all people age 16 and older with no postsecondary education (i.e., high school or less). To be clear, the majority of workers in this sample will not be directly affected, as many workers with high school or less are employed in jobs that pay above the minimum wage.[6] As a consequence, focusing exclusively on this

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[4] Consistent PUMAs (CPUMAS) are defined by IPUMS; they are aggregations of one or more PUMAs: https://usa.ipums.org/usa/volii/cpuma0010.shtml. PUMA boundary definitions change after each decennial each census; in the ACS, the new definitions were implemented starting in 2012. CPUMAS represent the smallest geographic units that are consistent across all the years in our sample.
[5] If wages are growing faster than inflation, this procedure may cause us to overestimate median wages, in turn underestimating the relative minimum wage and the minimum wage bite.
[6] At the same time, there is significant geographical variation in the proportion of the non-college workforce that is employed in low-wage jobs. A direct test of minimum wage exposure in each sample estimates the extent to which sample average wages are shifted. In these models, we find that higher minimum wages do significantly shift

broadly-defined sample might risk understating any disemployment effects. For this reason, we also include two alternative, more narrowly-defined estimation samples: individuals who have *not* completed high school and teens (age 16-19). Workers in these groups are more likely to be employed in low-wage jobs, suggesting a larger scope for potential disemployment effects, especially in high-impact coumas.

As a placebo group, we use people with a bachelor's degree or higher; this population is unlikely to work minimum wage jobs. For each of these groups, we calculate couma average hourly wages as well as employment rates. All dollar amounts are adjusted for inflation to 2016 dollars, unless otherwise noted.

Our main employment outcome variable is the employment to population ratio among people aged 16-70. When constructing this variable, we count as employed every individual who worked at some time during the reference year. We also include measures of weeks worked, full-year work (50-52 weeks worked in the reference year), usual weekly hours and a binary indicator for full-time work (usual weekly hours of 35 hours or more). For these variables, we calculate the couma average over the full sample in each population of interest, as well as average values conditional on working (excluding people with zero wage income). Finally, in order to capture effects of minimum wages on households at the lower end of the earnings distribution, we include measures of household and child poverty rates. We also construct indicators for independent contractors, using data on class of worker for individuals who were employed during the reference week. We follow standard practice in assigning workers independent contractor status if they are self-employed and non-incorporated.

Since higher minimum wages could expand the relevant labor market, especially in rural areas, we also measure commuting using ACS data on place of work for employed workers. In the 1-year ACS files, this information is available at a less fine-grained level of aggregation. Some place-of-work identifiers span multiple coumas –we are not able to determine commuting

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average wages in the sample of workers with high school or less, especially in high impact coumas, suggesting a non-negligible incidence of minimum wage work even in this broad sample.

status for workers who live in these coumas.[7] As a consequence, our commuting measure is defined for a subset of the sample (data is missing for 9.4 percent of couma-year observations, representing 4.9 percent of the population.)

These variables are then collapsed by couma and year, yielding a couma-by-year dataset of median wages, average wages and employment rate for various demographics, as well as household and child poverty rates. We merge the sample with data on state population, state unemployment rates and state GDP from the University of Kentucky Center for Poverty Research (UKCPR) database. Our main source of minimum wage data is the Vaghul and Zipperer (2016) minimum wage database: the effective minimum wage is the highest of the state and federal minimum wage. Importantly, we ignore sub-state (city and county) minimum wages.

We supplement the analysis of the ACS data with data from two additional sources: the QWI and the QCEW. Both of these datasets provide county-level data on jobs and earnings. Unlike the ACS, the QWI and the QCEW are assembled from administrative records submitted by employers rather than from household survey data. In the QWI, we define employment and monthly earnings based on employment at the beginning of each quarter. That is, we include workers who did not work the full quarter; restricting the sample to full-quarter workers could disproportionally exclude low wage workers who may be less attached to the labor force. In the QCEW, we use average employment rates over the three months of each quarter, as well as the average weekly wage.

Directly identifying counties with high exposure to the minimum wage (high Kaitz index or high bite) would ideally require county-level estimates of median hourly wages by year. As this data is not available, we instead assign each county's exposure status based on the couma-level exposure rates calculated using the ACS sample. About 99 percent of counties and 93 percent of the population are perfectly nested within coumas. For the counties not directly nested

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[7] The couma to place-of-work couma crosswalks are constructed using puma-to-place of work puma crosswalks provided by IPUMS: https://usa.ipums.org/usa/resources/volii/puma_migpuma1_pwpuma00.xls and https://usa.ipums.org/usa/volii/00pwpuma.shtml

within coumas, we instead calculate county-level exposure as the population-weighted averages of couma-level exposure rates.[8]

2.2 Empirical models

The period we study contains substantial variation in state and federal minimum wage policies. All the states in our sample experience one or more changes to the statutory minimum wage over the 2004-2017 period. Our analysis then has no untreated control group. Instead, we achieve identification by leveraging the differential timing of the minimum wage changes. In our empirical analysis, we implement a difference-in-difference framework under the assumption that states that do not change their minimum wage in a given calendar year provide a counterfactual for states with policy change (Lafortune et al. 2018). More precisely, we use the variation in state policies to estimate a set of regressions of couma level wages and employment, controlling for area and year fixed effects, as well a parsimonious set of couma and state-level control variables.

In order for the difference in difference research design to estimate the causal effects of the minimum wage, we require the parallel trends assumption to hold. That is, conditional on the covariates in the regression model, the residual variation in minimum wages should be uncorrelated with underlying trends in employment and earnings. Our models control fully for couma-specific factors that are constant over time, as well as aggregate changes to the economy. However, the models could still yield biased estimates if the timing of minimum wage changes is correlated with unobserved trends in outcomes. Such bias could be present, if, for example, states are more likely to pass minimum wage legislation when the economy is doing well.

The parallel trends assumption cannot be tested directly, as it is a statement about counterfactuals. However, there are testable implications. In our analyses, we therefore implement specification tests to assess the likelihood that the parallel trends assumption holds in our settings. First, we test for pre-trends in outcomes, by estimating a set of scaled event study models (Finkelstein et al. 2016). Second, we estimate a set of placebo regressions on a sample of college educated workers. Third, we follow Dustmann et al. (2020) in estimating a set of

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[8] We construct the couma-county crosswalks and population weights using PUMA-county crosswalks obtained from the Missouri Census Data Center's geographic correspondence engine (geocorr) tool. http://mcdc.missouri.edu/applications/geocorr.html

augmented difference-in-differences specifications accounting for differential trends in outcomes.

The event study models provide a simple way to assess pre-trends in outcomes. If the parallel trends assumption holds, we would expect outcomes to trend in parallel in the years leading up to minimum wage changes. The event study specification is also an attractive specification in our context, where we implement a difference-in-differences framework with staggered treatment timing with no never-treated units. In such settings, Goodman-Bacon (2019) shows that the difference-in-difference specification could yield biased estimates in the presence of the heterogeneous treatment effects; the event study specification may be more robust in this case.

The intuition behind the event study specification is simple: Increases in the minimum wage should not have any effects on earnings or employment in the years leading up to the policy change. Put differently, if wages and employment rates rise in the years leading up to minimum wage increases, the estimates from the generalized differences risk being biased upwards, reflecting unobserved state trends rather than the policies we study.

To define events, we first include all year-on-year increase in the applicable minimum wage (higher of state and federal) of 25 cents or more. Next, we require that the minimum wage did not change for at least two years leading up to the event – this requirement ensures that we are able to assess pre-trends. We do allow for additional changes to the minimum wage in the years following the initial increase, as minimum wage policies are typically phased in over several years. To ensure we have enough post-periods to adequately capture effects of policy changes, we exclude events occurring after 2014. For each event, we include up to four years of data before and after the event year, although we do not require the sample to be balanced in event time.[9]

These criteria yield a total of 51 events: 46 states experience at least one qualifying event, and 5 states experience two events during the sample period (see Appendix table A for a full list). The differential timing of these policy changes will be the primary source of variation in

[9] While a longer pre-period might be desirable for assessing long run differential trends in outcomes, we are unable to do so because the ACS is only available starting in 2005.

our empirical models. Crucially, the federal minimum wage increase in 2007-2009 will be a qualifying event for most of the states; the exceptions are a handful of states that were already above the new federal minimum. This pattern allows us to estimate effects of minimum wage increases in regions with relatively low minimum wages (and low state median wages).

For each event, we define δ_c as the change in log min wage over the event window.

$$\delta_c = logmw_c^{max} - logmw_c^{min}$$

We can write the augmented event study specification as

$$y_{ct} = \theta_c + \theta_t + X_{ct}\beta + \sum_{k=-3,k\neq 1}^{4} \left(\pi_{k(c,t)} \times \delta_c\right)\rho^k + \varepsilon_{ct} \tag{1}$$

The models control for couma-event and year specific intercepts as well as a vector of state and couma characteristics: the models control for the state unemployment rate, state GDP per capita, and log couma population.[10] The primary coefficients of interest is the parameter vector ρ, which captures the expected change in outcomes around the time of the policy change. As these coefficients are only identified relative to each other, we follow convention and set the last pre-increase period as the reference category, i.e. $\rho^{-1} = 0$.

In the absence of a control group, the event study model requires one additional normalization for identification (Borusyak and Jaravel 2017, Schmiedheiny and Siegloch 2019). We follow the standard approach in the literature and bin event-time at the earliest pre-period, that is, we set $\rho^{-4} = \rho^{-3}$.

To reiterate, our difference-in-differences research design relies on the assumption that states that do not increase their minimum wages in a given year provide a valid counterfactual for states that do. If this holds, we would expect no systematic differential trends in wages and employment in the years leading up to minimum wage increases. That is, for $k = 0$, the estimated event time coefficients should be small and close to zero for all years leading up to the

[10] For states with two events, we include a separate intercept for each of the two events. Similar models with couma-event fixed effects rather than state-event fixed effects yield nearly identical results, which is as expected given that minimum wage policies studied vary only at the state level (our analysis ignores county and city minimum wage ordinances).

minimum wage increase. Meanwhile, for $k \geq 0$, any positive (negative) effects of the minimum wage should show up as a discontinuous jump (drop) in the estimated event time coefficients. Qualitatively, we expect effects to show up as a discontinuous shift at time 0 (the year of the initial increase), potentially increasing in magnitude over the post-period reflecting gradual phase-ins of minimum wage policies. In this regression model, the event time indicators π_k are interacted with our measure of the aggregate change in the log minimum wage over the event window. The estimated sizes of the jump therefore indicate the (semi-) elasticities of employment and wages with respect to the minimum wage.

Following the standard approach in the literature, we also estimate generalized difference-in-differences models on the form

$$y_{ct} = \theta_c + \theta_t + X_{ct}\beta + logmw_{ct}\gamma + \varepsilon_{ct} \tag{2}$$

The econometric models presented in equations (1) and (2) form the basis of our empirical analysis. However, the key focus of this brief is not the average wage and employment impacts of higher minimum wages. Rather, we wish to estimate how impacts vary across localities with different expected impacts. For each of the events in the sample, we calculate two couma-specific measures of expected impact. First, we follow Cengiz et al. and define the event-specific Kaitz index as the ratio of the minimum wage at the end of the event window to the couma median wage in the last pre-increase year.[11] Second, we calculate the bite as the share of workers in the final pre-increase year whose hourly wage is below the new minimum wage. These two metrics will then be used to classify the localities in the events sample into subsamples; models (1) and (2) are then estimated separately on each group.

2.3 Descriptive statistics

Relative minimum wages and minimum wage bites vary considerably more among coumas than they do among states. We demonstrate this point in Figure 1, which uses federal and state minimum and median wage data to plot the distribution of relative minimum wages and minimum wage bites across coumas. The relative minimum wages and minimum wage bites are displayed at the state (grey bars) and couma levels (white bars), respectively.

[11] Hyman Kaitz, a statistician at the Bureau of Labor Statistics, is credited with introducing this ratio into the minimum wage literature.

As Figure 1a shows, the distribution of couma-level relative minimum wages following minimum wage increases is considerably wider than the distribution across states. While state-level relative minimum wages vary between .35 and 61, couma-level relative minimum wages vary between .26 and .82. Importantly, the maximum couma-level relative minimum wage is 35 percent higher than the maximum state-level relative minimum wage, and more than one-third higher than in Cengiz et al. While the state-level relative minimum wage after minimum wage increases exceeds 0.50 for less than 33 percent of Americans, a significantly larger share – 56 percent -- live in areas where the couma relative minimum wage reaches 0.50 or higher. Our empirical analysis leverages this variation to analyze how employment responds to minimum wage changes at these higher indices of minimum to median wages.

Figure 1b shows comparable histograms for the share of workers below the new minimum wage when a minimum wage is increased—the minimum wage's bite. Once again, the variation in the minimum wage bite associated with the minimum wage events is substantially greater across coumas than among states.

One of our key metrics of the expected impact of minimum wage increases is the couma relative minimum wage, defined as the ratio of the new minimum wage to the pre-increase median. Figure 2 explores the relative importance of variations in the minimum wage and the median wage in determining the ratio of the two measures across decile bins (labeled KR deciles in the figure, for Kaitz ratios). Minimum wage levels are essentially the same in all the Kaitz ratio deciles, while median wages fall monotonically with increases in the Kaitz ratio decile. The variation in relative minimum wages between high and low Kaitz ratio coumas appears to come almost entirely from variation in median wages, and not from minimum wage policy.

Figures 3a and 3b provide maps of relative minimum wages and average bites for each of the minimum wage events in our sample.[12] Figure 3a presents couma-level relative minimum wages for each minimum wage event in the sample period. Figure 3b shows the bite, the share below the new minimum wage, for each of the minimum wage events in the sample period. For both metrics, the highest impact areas have the darkest colors. A comparison of figures 3a and 3b

[12] For the five states that have two events over the sample period, the map shows the first event only. States that have no qualifying event are colored white. The map boundaries correspond to IPUMS-defined CPUMAs (consistent pumas): coumas that represent a single county with several cpumas are all assigned the same value.

indicates that the relative minimum wage and the minimum wage bite are highly correlated. The figures also show that coumas with the highest relative minimum wages and bites are not limited to one geographic area. Relative minimum wages are high in much of Arkansas, Florida, Kansas, Louisiana, Maine, Nebraska and Oklahoma, in much of western and southern Texas, and in much of the Pacific Northwest, including areas of California near the Oregon border. They are not as high in Alabama, Mississippi and Missouri.

To see this more clearly, Figure 4 ranks states according to their population in localities that are in the top quartile of relative minimum wages (upper panel, labeled Kaitz ratio in the figure) and bite (lower panel). While the highest shares of high relative minimum wage areas are found in two relatively low-wage, rural states (Montana and West Virginia), the overall picture is more mixed. For instance, California, a state with high average wages that is implementing a $15 minimum wage by 2022, has a higher share of the population living in high relative minimum wage localities than do both Mississippi and Alabama, two of the nation's poorest states.[13]

In Appendix A, we show similar graphs with the population share of each state that lives in areas with bites above 0.15 and 0.2, and the share that live in areas with relative minimum wages above 0.5 and 0.6, respectively. These figures show that for moderately high thresholds – 15 percent bite, 0.5 relative minimum wage – most states have at least some observations in the high impact sample. At the higher thresholds, the remaining sample includes a substantial but smaller number of states – 32 states have one or more couma-events where the bite is above 20 percent, while 25 states have one or more couma-events with a relative minimum wage higher than 0.6.

Table 1 presents summary statistics of the full sample as well as high and low impact coumas and counties. Compared to low impact coumas, high impact coumas tend to be more rural, more Hispanic, and have a smaller share of workers commuting out-of-couma. High impact coumas also have lower median wages.[14] In part, this difference in average wage levels reflects compositional effects: the share of college educated adults is lower in high impact areas. However, these regions also have lower earnings for workers with high school or less education;

[13] Note that the two measures do not always line up. The most extreme case is South Dakota, which consists of a single couma, where 100 percent of the population resides in high KR localities, while 0 percent of the population resides in high bite localities.
[14] We classify coumas as urban/rural using data from the United States Department of Agriculture – Economic Research Service https://www.ers.usda.gov/topics/rural-economy-population/rural-classifications/.

earnings in low-wage industries (retail and food service) are lower in high impact coumas as well.[15]

Appendix Figure A2 shows the distribution of blacks, Hispanics and college graduates by Kaitz ratio. High impact coumas have lower proportions of black workers. In contrast, the proportion of blacks is higher in low-wage states, especially those in the South. Our high sample of high-impact coumas includes many areas of California that are more populated by Hispanics than by blacks. Meanwhile, the proportion of college graduates in the workforce varies inversely with the relative median wage. Given our definitions of high and low impact coumas, these patterns are not surprising, since median wages and education levels are positively correlated.

3. Empirical results

We present first our main results using event study models, then show results of robustness tests and finally our results using generalized difference-in-difference methods.

3.1 Event-study models

Figure 5a presents estimated event study models of employment and earnings for individuals with high school education or less, aged 16-70. The panels on the left represent the effects of the minimum wage in localities with final relative minimum wages in the lowest quartile of the event sample.[16] In this sample, the inflation-adjusted indices of the minimum wage at the end of the event window to the median wage in the year before the minimum wage range between 0.26 and 0.46. The panels on the right present the effects in localities with relative minimum wages in the highest quartile; here the relative minimum wages range from 0.56 to 0.82. The two upper panels present results for wages. The two lower panels present results for employment.

For the low impact samples, the figures indicate upward trend in wages as well as downward pre-trends in employment. While we are hesitant to read too much into these pre-trends-- given the wide confidence intervals (which overlap zero in all cases), they could lead us to overestimate positive effects on wages and negative disemployment effects in low impact

[15] Industry-level earnings monthly earning figures are obtained by multiplying the average weekly wage from the QCEW by 52/12.

[16] Specifically, we define the event-specific relative median wages as the ratio of the highest minimum wage observed in the event window to the median wage in the final pre-event year.

samples. We find no pre-trends for the high impact sample (Q4) in either the ACS or QWI samples. In other words, the figures establish parallel pre-trends in our primary samples of interest.

Minimum wage legislation could have both direct and indirect impacts on wages. Workers who were initially paid a wage below the new minimum wage will receive a wage increase as employers comply with the new legislation – this is the direct effect. In addition, there could be spillover effects on wages above the new minimum wage, e.g. as firms seek to preserve existing wage structures. Unfortunately, our data does not allow us to confidently analyze the relative importance of each of these two channels. Several studies have quantified the role of such spillover effects; for example, Cengiz et al. (2019) estimate that around 40 percent of the estimated effect on wages reflects spillovers. Our estimated wage effects include both direct and spillover effects of the policy change.

As expected, we find that higher minimum wages tend to have the largest effects on wages of less-educated workers in areas where the relative minimum wage is higher. In the low relative minimum wage coumas, the wage increase at the time of the minimum wage change is small; while point estimates tend to be positive following the increase, these are indistinguishable from the slight pre-trend in wages for this sample. In the high impact regions, estimated event time coefficients tend to be close to zero in the years before minimum wage increases, indicating parallel pre-trends in wages in high impact areas. In these localities, we estimate a significant jump in event time coefficients for wages at time 0, when the new, higher minimum wage is implemented. The substantial increase in hourly wages in high impact areas following minimum wage increases is consistent with significant exposure to minimum wage work in this sub-sample. As a consequence, we might expect to see larger disemployment effects in these areas. However, this does not appear to be the case. Estimated employment effects do not differ between the high and low impact areas. The two lower panels show the effects on employment to population ratios. The absence of a jump at time 0 indicates that effects on employment are small to negligible in both samples. Again, we stress that the figure finds no indications of pre-trends in employment rates in high impact coumas – this finding of parallel pre-trends support the validity of our difference-in-differences research design.

As Appendix Table A1 indicates, many of our events are generated by the federal minimum wage increases from 2007 to 2009. Since this timing coincides with the onset of the Great Recession, our analysis might be affected by the sharp declines in employment that began during the Great Recession and extended into the first years of the economic recovery. Indeed, Clemens and Wither (2019) find that minimum wage effects during this period did generate negative employment effects. However, Zipperer (2016) presents evidence indicating that Clemens and Wither do not sufficiently control for differential effects of the Great Recession across industries and regions. We do not control for industrial and regional differences in our analysis and yet we do not detect negative employment effects.

To summarize to this point, the couma-level relative minimum wages appear to be informative of the impact of the minimum wage: In the population of adults with high school or less education, we find the largest wage effects in localities where the relative minimum wage is high. However, this wage effect does not translate to job loss, even in the highest quartile event subsample.

Thus far, our results indicate that higher minimum wages tend to increase wage rates in low-wage coumas, without reducing employment rates. These results suggest that we should expect a corresponding increase in incomes in low-wage areas. Figure 6, which plots the estimated event study models of poverty, indicates that this is indeed the case. The figure suggests some upward pre-trends in poverty in low impact areas. In high impact areas, however, the figures indicate flat pre-trends. In high relative minimum wage coumas, poverty falls significantly after minimum wage increases. In the low relative minimum wage coumas, we do not detect a significant effect on poverty rates, consistent with the lack of statistically significant wage effects in these areas.

3.2 Robustness tests

To assess the robustness of these findings, we estimate additional models that split the sample by the bite of the minimum wage rather than by the relative minimum wage itself. These models, which we presented in Figure5b, yield broadly similar conclusions. Wage effects are clearly larger in high bite localities. Employment effects are small overall; in the high bite subsamples they are close to zero.[17]

Higher minimum wages could also affect the probability that workers operate as independent contractors. This kind of adjustment could occur through demand side effects: if employers contract out tasks in an effort to cut back on higher wage costs, we might see higher incidence of contract work when minimum wages are higher. On the other hand, there could also be a negative supply effect, as higher minimum wages raises the return to wage work relative to self-employment for low wage workers. The top panel of figure 7 plots estimated event study models of contract work (defined as the share of the population age 16-70 who are independent contractors).[18] In high exposure coumas, we detect a reduced prevalence of contract work, consistent with workers shifting to wage work.

The analysis so far has failed to find evidence of significant employment effects, even for individuals living in high exposure coumas. However, coumas do not necessarily correspond to labor markets. In particular, for densely populated areas, cross-county commuting may be fairly common. If higher minimum wages reduce the number of available jobs in high exposure coumas, we could still see no net effect on employment levels if the displaced workers adjust by seeking work in neighboring coumas.

We address this possibility by estimating a set of event study models of cross-couma commuting. In principle, these models allow us to assess directly whether there is a differential uptick in out-commuting in high impact coumas following minimum wage increases. We present our estimated event study models of cross-couma commuting in the bottom panel of figure 7. The models show no discernible change in cross-commuting following minimum wage increases. That is, we find no evidence that higher minimum wages pushes workers living in low wage coumas to travel further in order to find work.

We next estimate a set of event study models of county-level earnings and employment outcomes using aggregate data from the QWI and the QCEW. These datasets classify jobs using

[17] In the high bite subsamples, the coefficients on employment tend to be negative, though not statistically significant, in the post-period. However, there is a negative pre-trend in employment in this sample, indicating that this result represents a differential trend rather than a causal impact of policy change.

[18] Figure 7 indicates flat pre-trends for both independent contracting and commuting.

the location of the establishment rather than the worker's residence. As a consequence, we may be able to capture effects on job loss in the presence of cross-couma commuting. In addition, these records represent a nearly complete census of establishments, which could allow us to estimate effects with greater precision. Unlike the ACS, the QCEW and the QWI both include quarterly data. In the following, we focus on a set of annualized event study models. Quarterly event study models, presented in Appendix C, yield very similar results.

While the QWI does not identify individual workers, it does report data by education category. Figure 8 shows estimated event study models of log monthly earnings and employment-to-population ratios for workers with high school or less education in counties in the first and fourth quartile of the Kaitz ratio distribution. Consistent with the patterns in Figure 5, Figure 8 suggests an upward pre-trend in wages in low-impact areas; unlike figure 5, the QWI also indicates an upward employment pre-trend in low impact areas. Reassuringly, these event study models again establishes flat pre-trends for non-college wages and employment in high impact areas.

As the figure indicates, higher minimum wages do not significantly shift monthly earnings for workers in either high or low impact localities. Figure 8 fails to detect any effect on employment for non-college workers – a reassuring result given the lack of wage effects. Results are similar when splitting the sample by bite results are similar if we instead rank counties by the "bite" rather than the Kaitz ratio, and if we exclude workers with a high school diploma.

The lack of an effect on average earnings for non-college workers may seem puzzling, given the significant impact on hourly wages in the ACS data. This discrepancy may result from the different earnings measures in the household and establishment datasets. Relative to the hourly wage variable we constructed using the ACS, the QWI's measure of monthly earnings places more weight on the wages of full-quarter full-time workers, who may be less likely to work minimum wage jobs.

Next, we use data from the QCEW to estimate models of earnings and employment in the food service and retail industries. These are the two sectors with the highest concentration of minimum wage jobs. We present the results in figures 9A and 9B. In high impact counties, higher minimum wages significantly raise earnings of food service and retail workers. Wage effects are not significant in low impact counties. Employment is not affected in either sample.[19] Splitting the sample by bite rather than Kaitz ratio again produces similar results.

3.3 Generalized difference-in-difference estimates

We next estimate a generalized differences-in-differences regression model on the event sample, replacing the event time coefficients with the contemporaneous values of the log minimum wage. To define high and low exposure localities, we again use two event-specific metrics: the relative minimum wage defined using the pre-increase median wage, and the bite—the share of workers with pre-increase wages below the new minimum. Specifically, we consider localities where the relative minimum wage is above and below .5, respectively, as well as a subsample of localities where the relative minimum wage is .6 or higher.[20] We also examine coumas with shares above and below 15 percent of below-minimum wage workers, as well as coumas where 20 percent or more of the workers were paid below the new minimum wage.

Table 2 shows the results from this exercise. Overall, these results are consistent with the findings from the event study models. For both metrics, higher minimum wages raise wages of workers with high school or less more in higher-exposure areas; in low exposure areas, we find no significant increase in wages. Looking at two subsamples with greater exposure to minimum wage work – people without a high school degree and teenagers – we find a similar pattern. While the wages of teens tend to increase in all localities, the size of the increase is larger in high impact coumas. Overall, this pattern indicates that the two metrics – the Kaitz ratio and the bite – capture variation in the impact of minimum wage policies.

Meanwhile, the model fails to find significant effects on employment for either of the non college-educated samples or for teens. This result holds both in the pooled sample of all localities (column 1) as well as across coumas. If high impact localities were less able to absorb the higher wage costs, we might expect employment effects to be more negative in high Kaitz ratio/high bite coumas; however, this does not appear to be the case.[21] We find no evidence of negative

[19] The figures suggests a slight upward trend in food service employment in high impact regions; this trend continues smoothly through the timing of minimum wage increases (i.e. no kink at time zero).
[20] That is, higher than the highest state-level relative minimum wage analyzed by Cengiz et al.
[21] This pattern holds even for individuals who are more exposed to minimum wage work: people without a high school degree and teens.

employment effect in high impact localities. This result holds even for individuals who are more exposed to minimum wage work: people without a high school degree and teenagers. In fact, comparing point estimates across columns (2) – (7) reveals a somewhat puzzling pattern: although not statistically significant, employment point estimates tend to be larger and more positive in the high impact coumas. We interpret this pattern as indicating possible differential employment trends in high couma areas. As we will show below, this pattern is consistent with the pattern found in the placebo sample.

To further compare these results with those in the literature, we calculate employment elasticities with respect to the minimum wage and own-wage elasticities, using the estimates from Table 2 as well as average employment rates in each subgroup. These estimates appear in Table A2. In the sample of all localities, our estimated own-wage elasticities for the three high impact groups range from -0.159 to 0.176. These are well within the range of estimates reported in the literature (see Dube 2019 and Harasztosi and Lindner 2019 for recent reviews).

For the college-educated sample, the models find no effects on wages or employment in the overall sample. This result accords with what we would expect, given the low exposure of this group to minimum wage work. Looking across coumas yields overall similar results for wages, with the exception of a marginally significant and small negative wage effect in the lowest Kaitz ratio subsample.

The placebo regressions find no significant effects on employment for college graduates in the full sample of high Kaitz ratio and high bite coumas (defined as above 0.5/0.15 respectively) or for high bite coumas. For the sample with the highest bites (above 0.2), the model finds a statistically significant positive effect on employment; in the sample with Kaitz ratios above 0.6 employment effects are marginally significant.[22] This finding suggests some possible misspecification in our models. Misspecification could result if changes in minimum wages are correlated with unobserved employment growth in the highest Kaitz ratio coumas. However, the sample of coumas with Kaitz ratios over 0.5 includes almost all the states, while the sample with Kaitz ratios over 0.6 includes about half the states.

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[22] The statistical significance of these results should be interpreted with some caution. We cluster standard errors on state; in the highest bite subsamples, we have only 25 clusters. With few clusters, we are likely to underestimate standard errors, as a result, the statistical significance of effects in this sample may be overstated.

The employment result for the more limited sample may therefore reflect some selection effects. Such selection effects could also account for the pattern of point estimates of employment effects for less educated workers becoming larger and more positive in high impact samples, as the estimated employment effects in the high bite samples are very similar across education levels. Finally, the last two rows show the effects on the poverty rate in the full population, as well as on child poverty: a higher minimum wage significantly reduces these measures in high exposure areas.

So far, the estimated models find no evidence of negative employment effects. However, these results could be misleading if employers respond to higher wages by cutting back on hours rather than by reducing head count. To address this possibility, we estimate effects of the minimum wage on hours and weeks worked; these models are estimated on the full sample of all people with high school or less as well as on the subsample of workers (that is, excluding non-workers). The results, presented in Table 3, indicate no significant negative effects on hours or weeks worked.[23]

Our analysis of aggregate county-level employment data found no effects of minimum wages on average monthly earnings of non-college workers on average. However, in high impact counties, wages in low wage industries increased significantly after minimum wage increases, with no corresponding drop in employment. The corresponding diff-in-diff models for these outcomes tell a broadly similar story: While monthly earnings for all non-college jobs are not affected, effects on food service and retail earnings are positive and significant, with the largest effects found in high Kaitz ratio areas. We also estimate wage and employment outcomes for blacks, Hispanics and women. The results, shown in Table A3, do not detect negative employment effects among any of these groups.

The event study models, presented in the previous sections, did not find evidence of significant pre-trends in wages or employment in high impact areas. To further assess the role of differential trends in high impact areas, we follow Dustmann et al (2020) and estimate a set of

[23] In fact, the results suggest possible positive intensive margin effects in the lowest bite coumas: conditional on working, hours and weeks worked both increase in this subsample.

specifications accounting for differential trends in outcomes across localities.[24] Results from these specifications, presented in Appendix Table A4, are consistent with our baseline results: we find significant effects on wages, with no significant impact on employment.[25]

To summarize, the generalized differences-in-differences models indicate that while higher minimum wages raise wages more in high-exposure areas, we do not see a corresponding reduction in employment or hours. Importantly, this result holds even in areas where the exposure rates are very high, including localities where more than one in five workers are directly affected by the minimum wage.

4. Summary and conclusions

We use sub-state variation in median wages to array local areas according to the likely effects of minimum wages. Doing so substantially expands the range of relative minimum wages and minimum wage bites beyond the levels observed with state-level data. Our sample of relative minimum wages in low-wage areas encompasses relative minimum wages as high as .82, well above the .59 maximum in previous minimum wage research.

Using data from the American Community Survey, the Quarterly Workforce Indicators, and the Quarterly Census of Employment and Wages, we estimate both event study and generalized difference-in-difference models to analyze the effects of minimum wages on wages, employment and poverty in areas with low and high relative minimum wages (low median wages) and with low and high minimum wage bites. We conduct these analyses among a range of high-exposure groups (those with high school education or less, teens, and workers in low wage industries). The results are similar across all these groups and across the datasets. We find that minimum wages increase wages more in the high impact areas, validating our methodological approach. We do not detect that minimum wages decrease employment or hours in low or high impact areas. Minimum wage increases do, however, reduce poverty rates among households and children.

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[24] The three specifications augment the baseline regression model of equation (2) with (a) state-specific linear time trends, (b) linear time trends interacted with baseline couma characteristics and (c) calendar time fixed effects interacted with baseline couma characteristics. The models are estimated on coumas with KR>0.5 as well as coumas with bite > 15 percent.

[25] If anything, reductions in poverty are more significant in these specifications.

These results have implications for the policy debate on whether a federal $15 minimum wage should include exemptions for low-wage areas.

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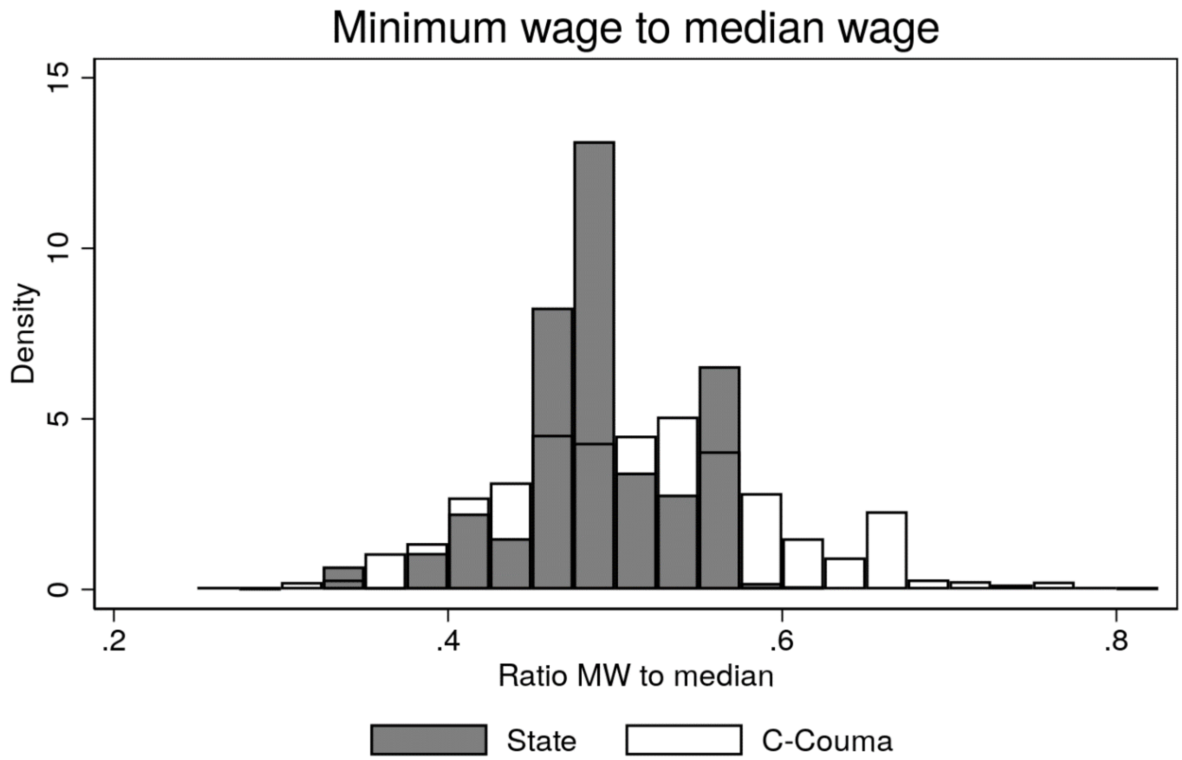
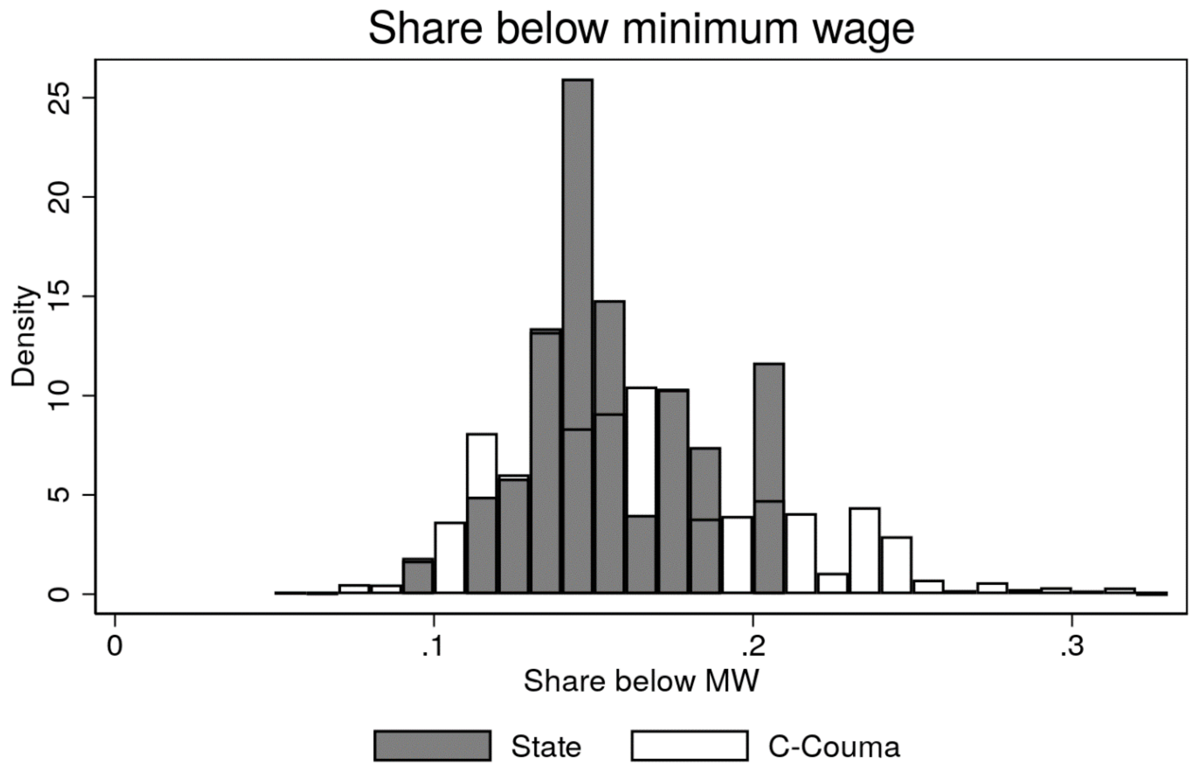
Figure 1a Histograms of ratio of state minimum wage to state and couma median wages



Figures plots the ratio of state minimum wage to couma and state median wage.
Source:ACS (2005-2017) and Vaghul and Zipperer (2016)

Figure 1b Histograms of share below new minimum wage, for states and coumas



Figures plots the estimated population share with hourly wages below the (new) minimum.
Source:ACS (2005-2017) and Vaghul and Zipperer (2016)

Figure 2 Source of variation in the relative minimum wage by decile

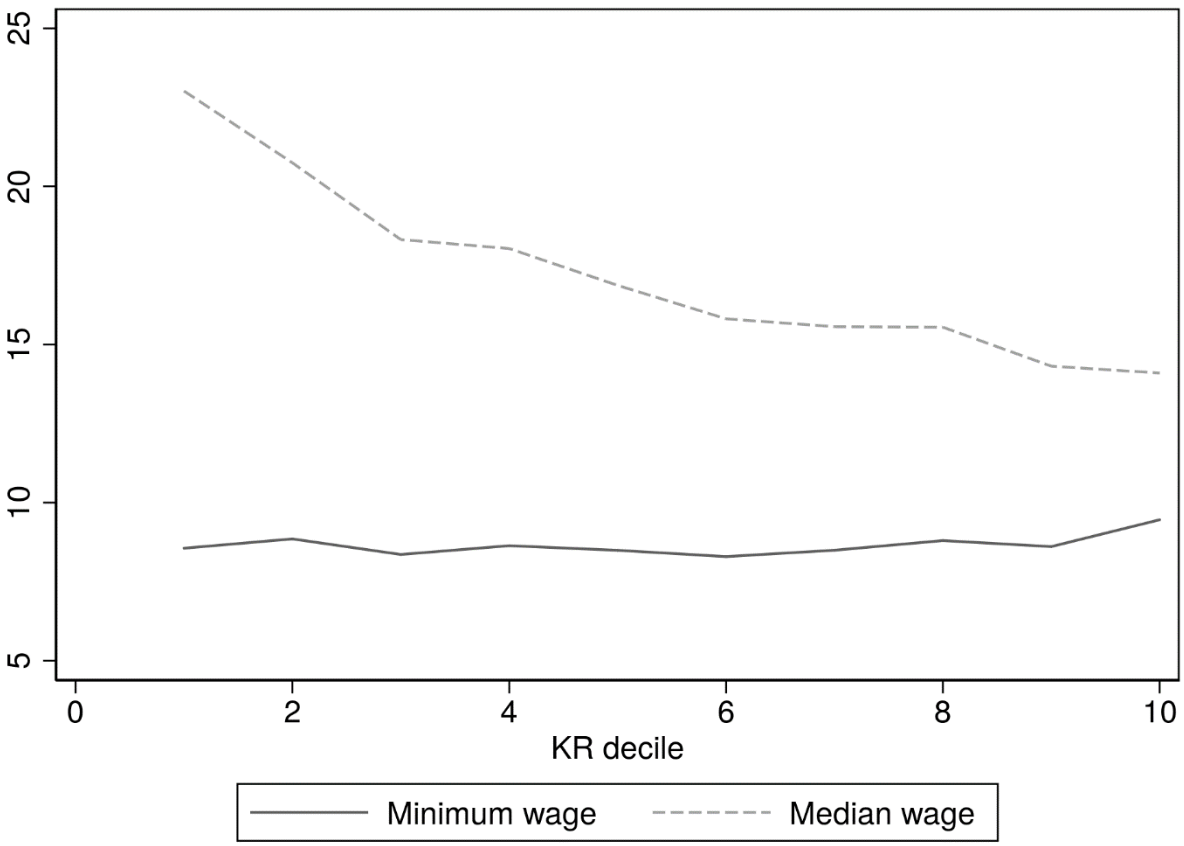
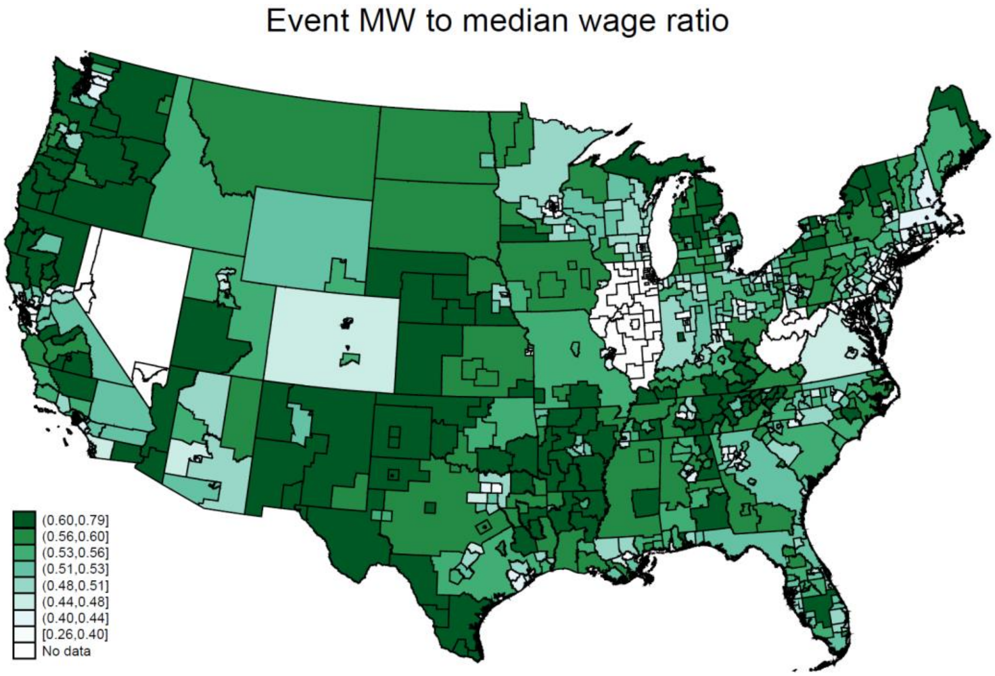


Figure 3 Relative minimum wage and minimum wage bite maps

a. **Relative minimum wages**



b. **Share below new minimum wage**

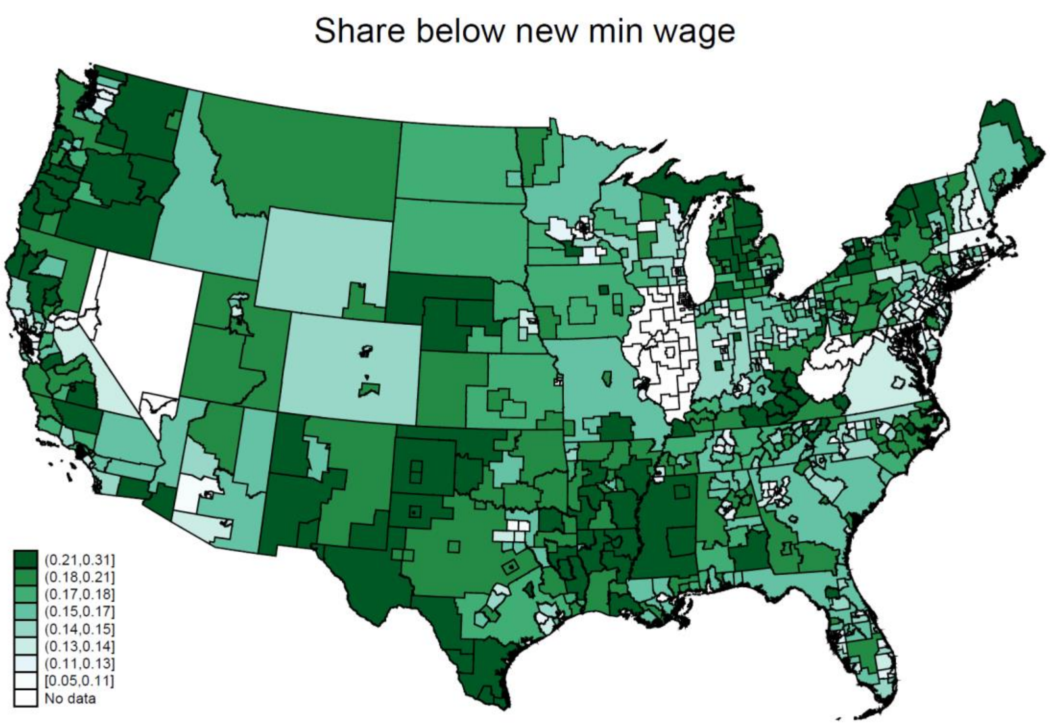
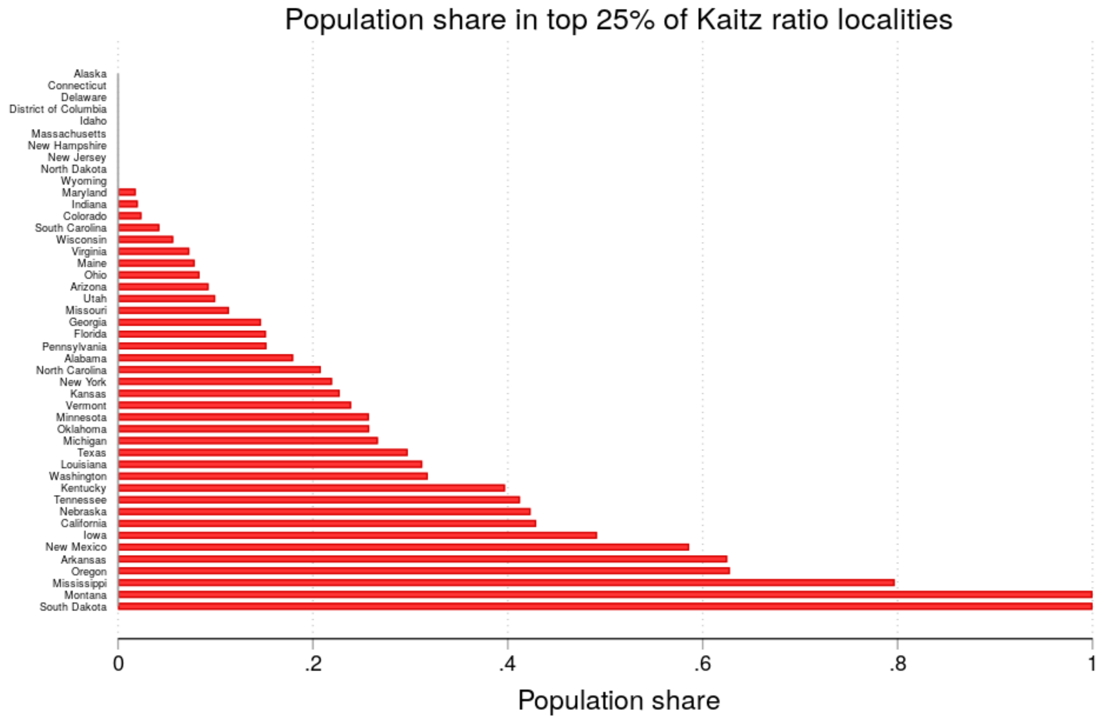


Figure 4 Population share in top quartile impact coumas



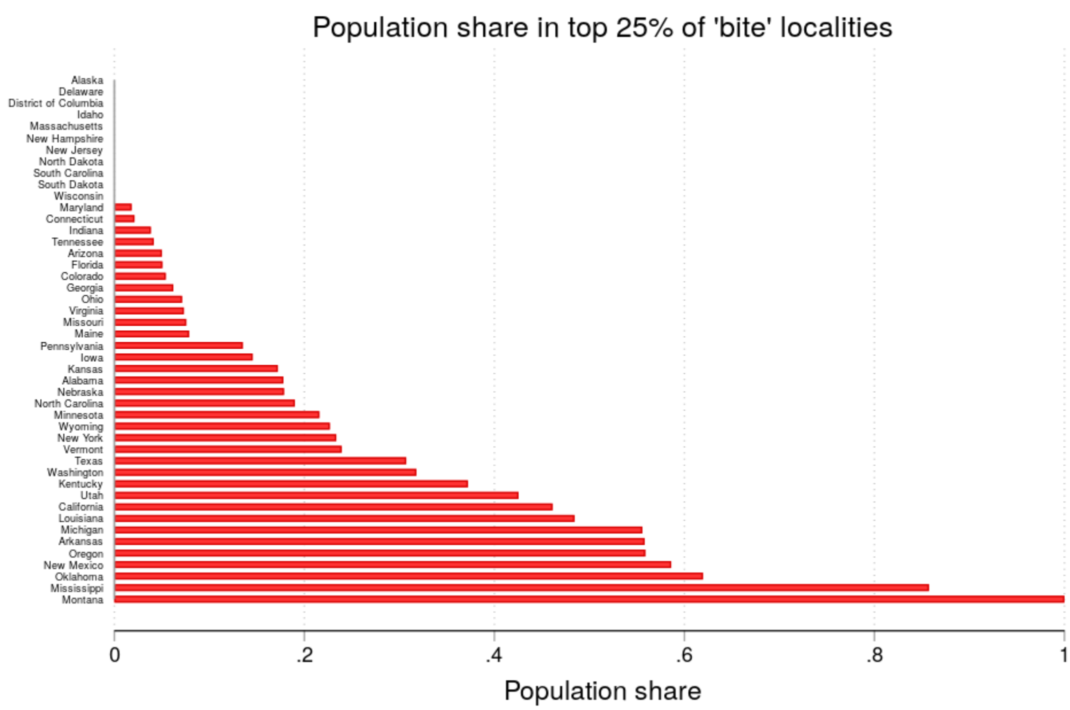
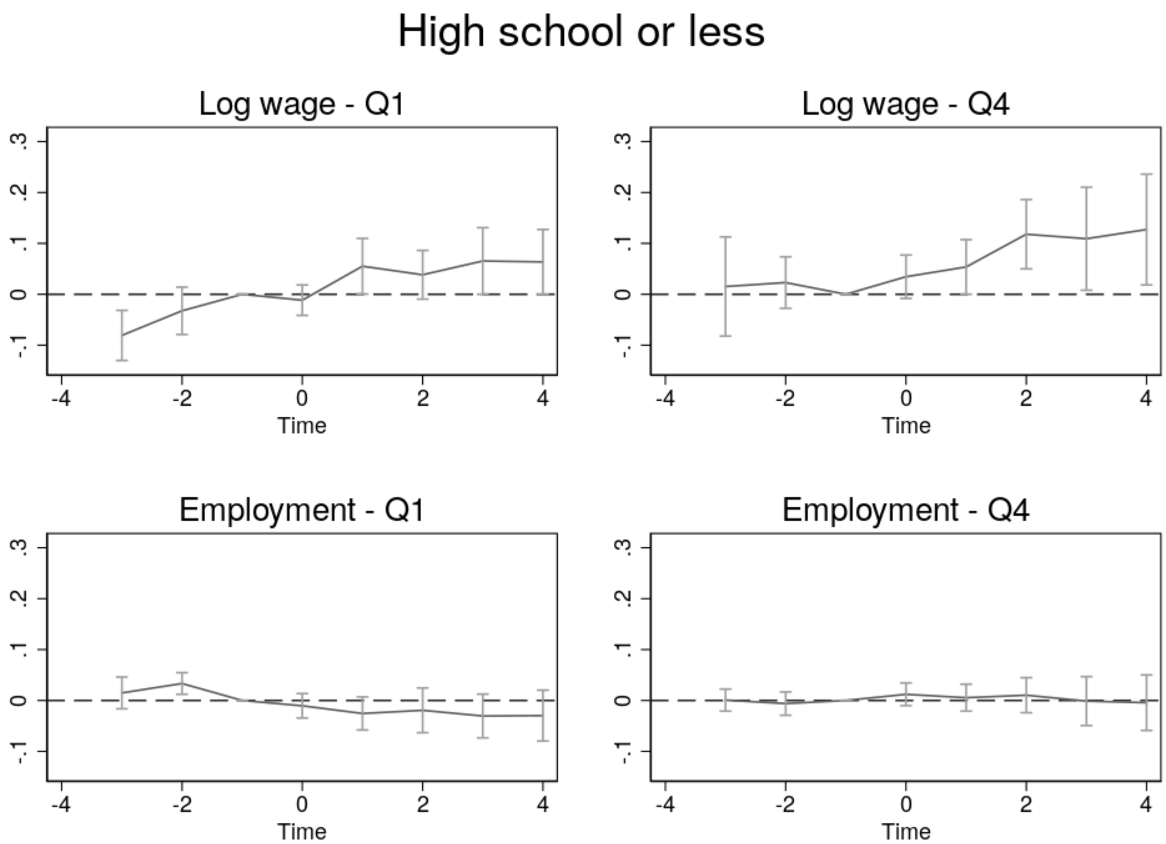
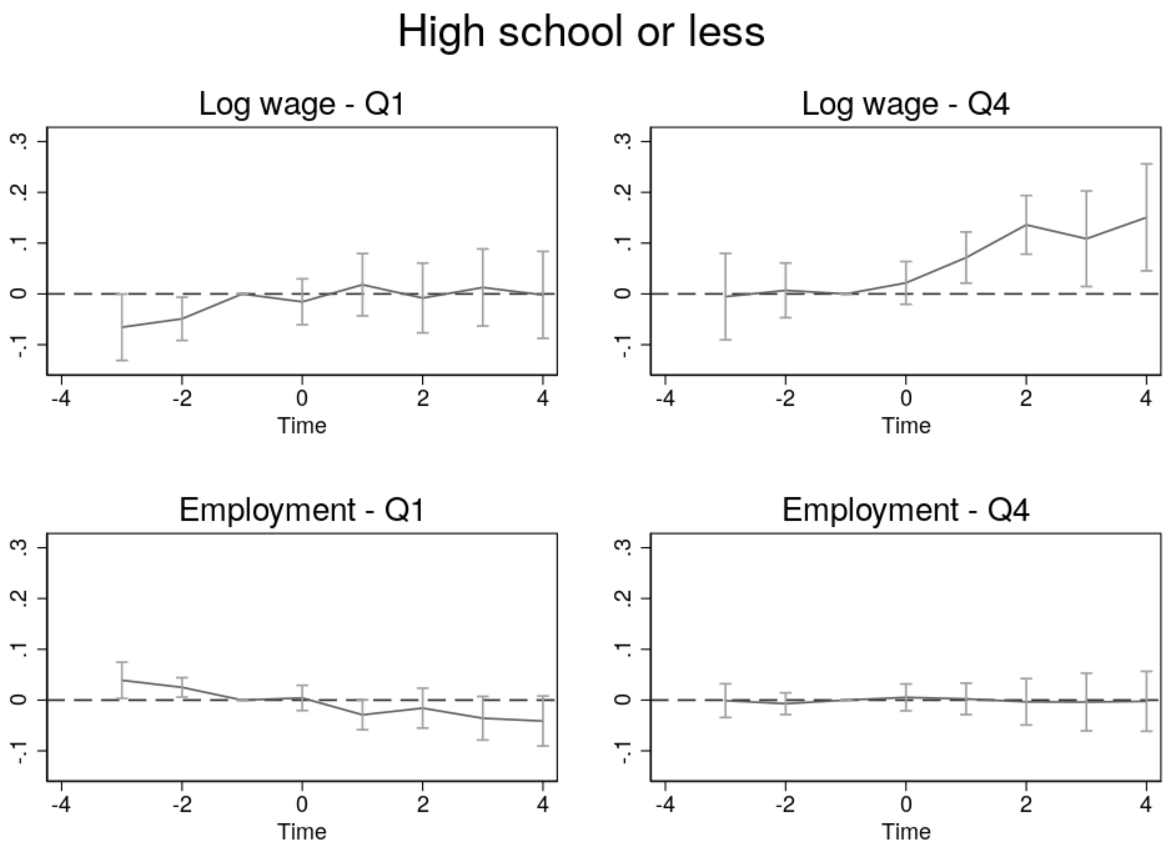


Figure 5a Event study models by relative minimum wage



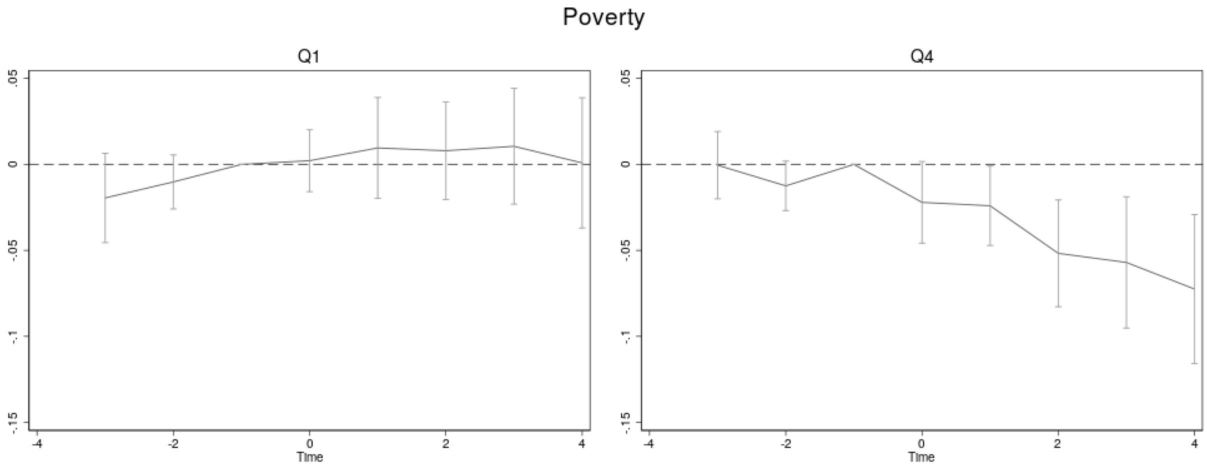
Note: Figure shows event study models of log wage and employment, estimated on the sample of people age 16-70 with high school or less, by quartile of the couma relative minimum wage distribution.

Figure 5b Event study models by bite

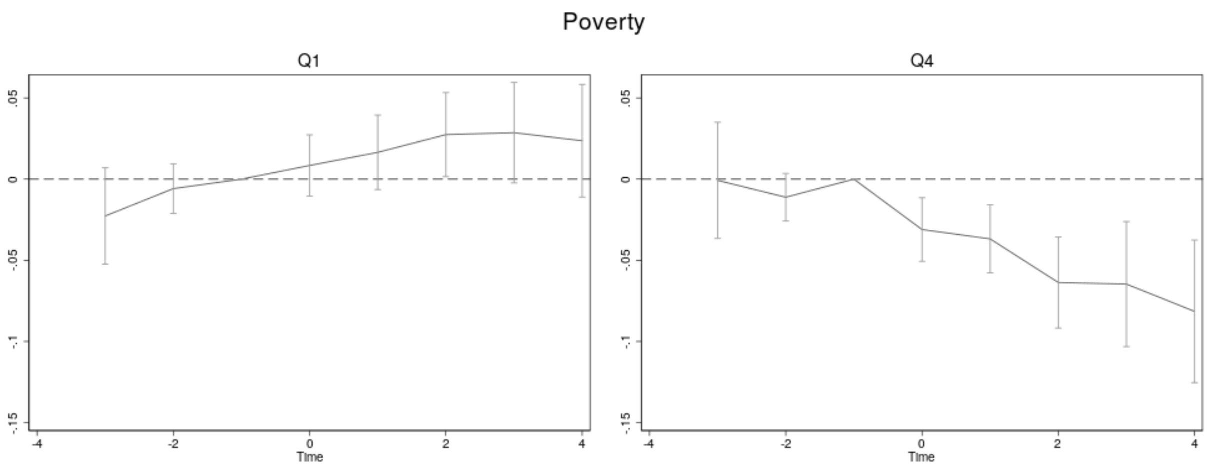


Note: Figure shows event study models of log wage and employment, estimated on the sample of people age 16-70 with high school or less, by quartile of the distribution of share below new minimum wage

Figure 6 Event study results, poverty



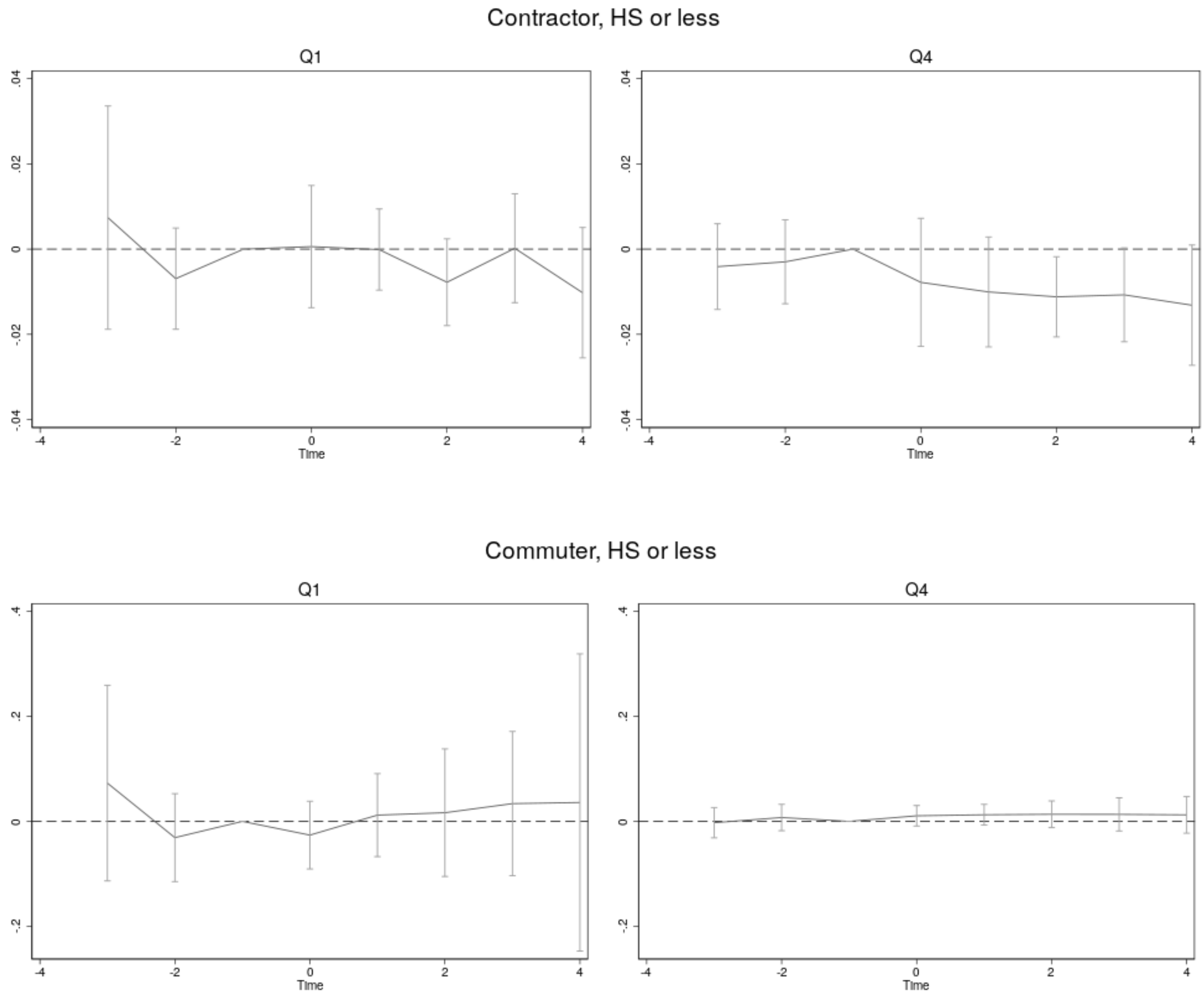
(a) Kaitz ratio quartiles



(b) Bite quartiles

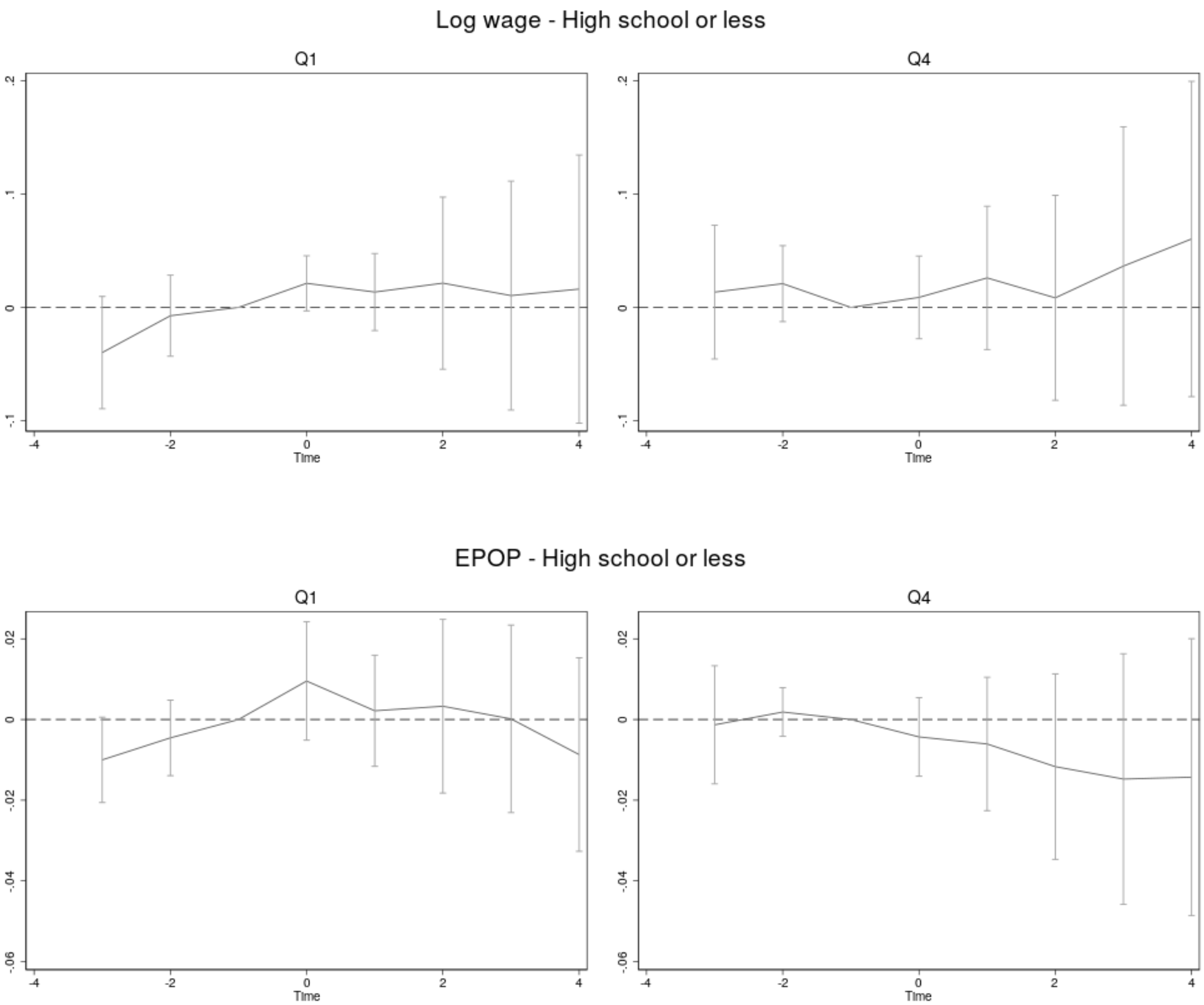
Note: Figure plots estimated event study coefficients from equation (1) estimated on coumas in the top and bottom quartiles of the relative minimum wage (Kaitz ratio)/"bite" distribution. The dependent variable is an indicator variable equal to one for people in households with incomes below the federal poverty line.

Figure 7 Independent contractor and commuting status, by quartile of Kaitz ratio



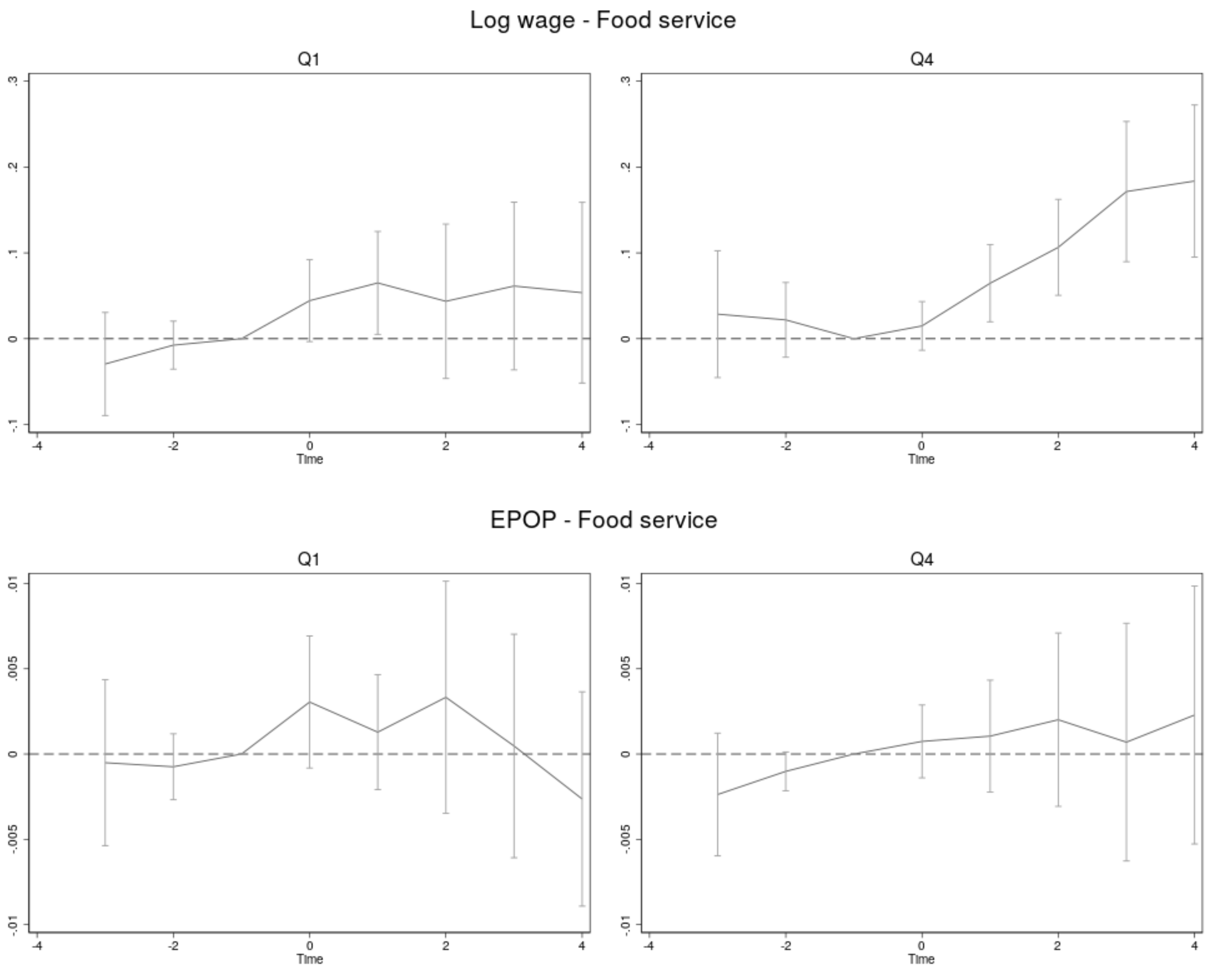
Note: Figure plots estimated event study coefficients from equation (1) estimated on coumas in the top and bottom quartiles of the relative minimum wage (Kaitz ratio) distribution. The dependent variables are an indicator variable equal to one for adults who worked

Figure 8 Wage and employment for non-college workers, by Kaitz ratio quartiles, QWI



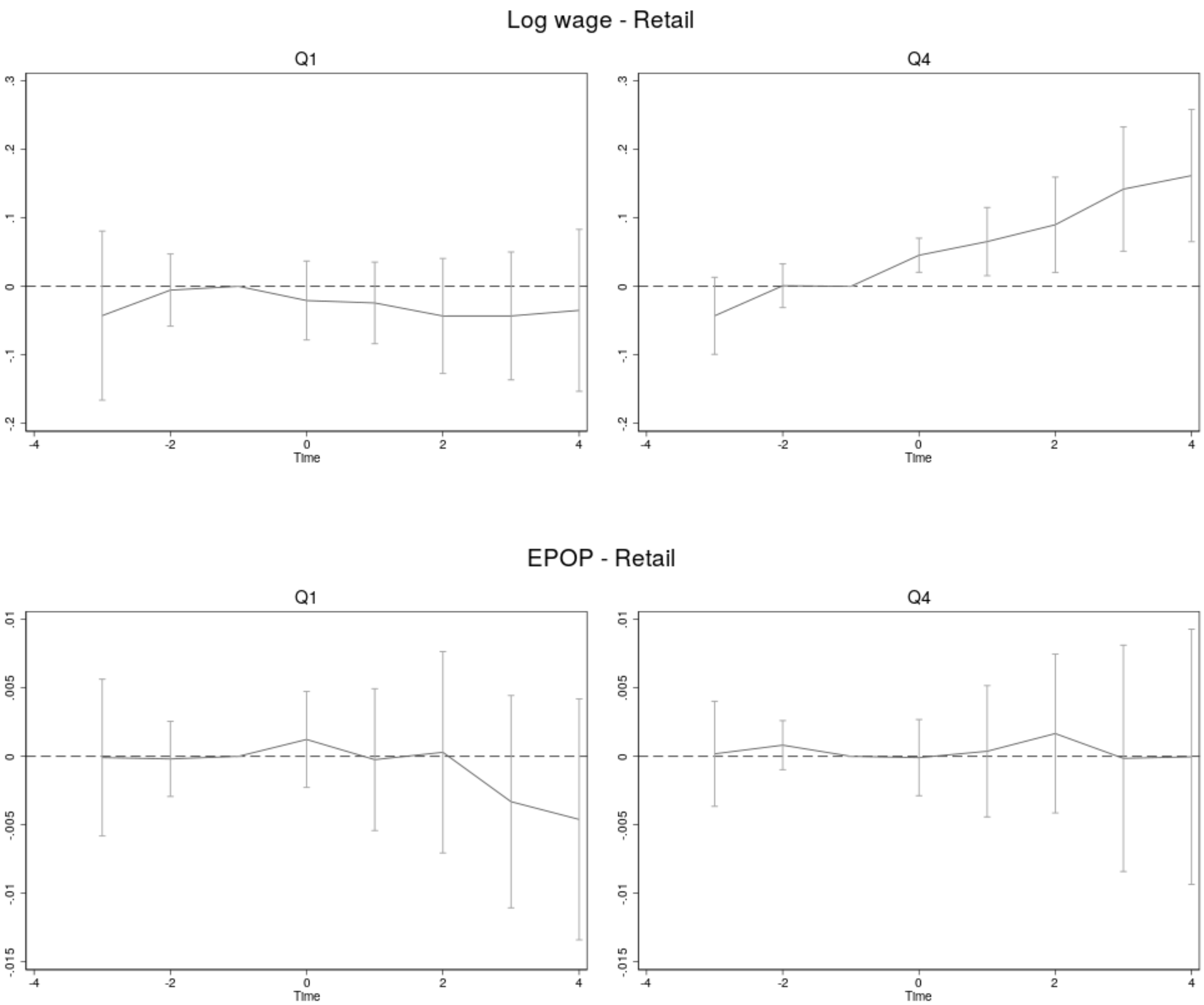
Note: Figure plots estimated event study coefficients from equation (1) estimated on coumas in the top and bottom quartiles of the relative minimum wage (Kaitz ratio) distribution. The dependent variables are log monthly earnings (upper panel) and employment to population ratio, defined as the ratio of employment of workers with high school or less education over the county level population ages 16-70. Source: QWI.

Figure 9A Results by industry Kaitz ratio quartiles – food service



Note: Figure plots estimated event study coefficients from equation (1) estimated on coumas in the top and bottom quartiles of the relative minimum wage (Kaitz ratio) distribution. The dependent variables are log average weekly wage (upper panel) and employment to population ratio, defined as the ratio of employment in the food service industry to the county level population ages 16-70. Source: QCEW.

Figure 9B Results by industry Kaitz ratio quartiles – retail



Note: Figure plots estimated event study coefficients from equation (1) estimated on coumas in the top and bottom quartiles of the relative minimum wage (Kaitz ratio) distribution. The dependent variables are log average weekly wage (upper panel) and employment to population ratio, defined as the ratio of employment in the retail industry to the county level population ages 16-70. Source: QCEW.

Table 1 Summary statistics

	(1) All	(2) LTMW Q1	(3) LTMW Q4	(4) KR Q1	(5) KR Q4
Minimum wage	7.846	7.803	8.179	7.928	8.073
Median wage	17.394	20.477	15.262	21.266	14.848
Relative minimum wage	0.462	0.388	0.540	0.377	0.546
Share below new MW (bite)	0.165	0.116	0.226	0.120	0.220
Share metro	0.855	0.973	0.757	0.984	0.691
Black	0.116	0.120	0.094	0.108	0.086
Hispanic	0.186	0.135	0.276	0.140	0.268
Share college	0.289	0.372	0.238	0.394	0.221
Commuter	0.228	0.295	0.158	0.313	0.154
Commuter max HS	0.203	0.252	0.149	0.266	0.148
Contractor	0.054	0.051	0.057	0.051	0.060
Contractor max HS	0.052	0.048	0.055	0.048	0.058
Employment	0.690	0.728	0.649	0.730	0.649
Employment max HS	0.609	0.638	0.573	0.636	0.577
Log wage	2.771	2.914	2.688	2.962	2.658
Log wage max HS	2.501	2.571	2.458	2.593	2.445
Poverty (all)	0.164	0.120	0.205	0.115	0.206
Child poverty	0.207	0.149	0.259	0.137	0.261
EPOP max HS	0.175	0.189	0.164	0.181	0.165
EPOP food service	0.043	0.049	0.039	0.048	0.038
EPOP retail	0.068	0.077	0.062	0.076	0.062
Monthly earn max HS	2997	3294	2773	3422	2707
Monthly earn food service	1377	1544	1292	1580	1260
Monthly earn retail	2494	2755	2375	2819	2328

Note: LTMW Q1 (Q4) shows summary statistics of coumas/counties in the first (fourth) quartile of the "bite" distribution (share population earning below the minimum wage). KR Q1 (Q4) represent coumas/counties in the first (fourth) quartile of the relative minimum wage/Kaitz ratio distribution. EPOP is defined as the ratio of cell level (education/industry) employment to the full population age 16-70. Sources: ACS/QCEW/QWI. Observations weighted by estimated population.

Table 2 Wage and employment effects: generalized difference-in-differences estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Localities:	All	KR<50%	KR>50%	KR>60%	LTMW<15%	LTMW>15%	LTMW>20%
Sample: Less than high school							
Log wage	0.114***	0.0818	0.132***	0.175**	0.0601	0.156***	0.180**
	(0.0370)	(0.0489)	(0.0371)	(0.0809)	(0.0504)	(0.0332)	(0.0676)
Employment	0.0109	-0.00864	0.0239	0.0413	0.00102	0.0145	0.0436
	(0.0270)	(0.0253)	(0.0381)	(0.0596)	(0.0291)	(0.0399)	(0.0629)
Sample: High school or less							
Log wage	0.0610**	0.00162	0.108***	0.179***	0.0203	0.0865***	0.146**
	(0.0271)	(0.0224)	(0.0333)	(0.0601)	(0.0259)	(0.0312)	(0.0662)
Employment	-0.000074	-0.0185	0.0185	0.0375	-0.0152	0.0137	0.0303
	(0.0190)	(0.0178)	(0.0245)	(0.0400)	(0.0174)	(0.0252)	(0.0313)
Sample: Teens							
Log wage	0.161***	0.109*	0.209***	0.325**	0.109	0.210***	0.230*
	(0.0527)	(0.0599)	(0.0679)	(0.122)	(0.0645)	(0.0655)	(0.126)
Employment	-0.0262	-0.0731	0.0297	0.0139	-0.0270	-0.0186	0.00341
	(0.0358)	(0.0460)	(0.0344)	(0.0521)	(0.0489)	(0.0391)	(0.0587)
Sample: BA+							
Log wage	0.0143	-0.0319*	0.0450	-0.0359	-0.0232	0.0299	-0.0694
	(0.0211)	(0.0162)	(0.0366)	(0.0643)	(0.0192)	(0.0310)	(0.0552)
Employment	-0.00391	-0.0128	0.00918	0.0494*	-0.00724	0.00430	0.0521**
	(0.00993)	(0.00949)	(0.0157)	(0.0273)	(0.0119)	(0.0142)	(0.0232)
Sample: all							
Poverty	-0.00486	0.00309	-0.0162	-0.0878**	-0.00272	-0.0122	-0.0653**
	(0.0102)	(0.00852)	(0.0174)	(0.0364)	(0.0104)	(0.0153)	(0.0316)
Child poverty	-0.00632	0.00514	-0.0194	-0.131**	0.00733	-0.0256	-0.0754
	(0.0164)	(0.0172)	(0.0283)	(0.0579)	(0.0189)	(0.0236)	(0.0543)
Observations	5887	2213	3674	1225	2196	3691	1384
Couma-events	743	281	462	156	277	466	177

Note: All models control for couma by event and year fixed effects, log couma population, log state unemployment rate and log state GDP. Observations weighted by population. * p<0.10 ** p<0.05 *** p<0.01

Table 3 Hours and weeks worked

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Localities:	All	KR<50%	KR>50%	KR>60%	LTMW<15%	LTMW>15%	LTMW>20%
Sample: All HS or less							
Weeks worked	0.292	-0.401	0.979	2.646	0.0907	0.590	1.614
	(0.920)	(0.645)	(1.384)	(2.410)	(0.678)	(1.373)	(2.095)
Full year work	0.00753	0.00000640	0.0124	0.0367	0.0151	0.00162	0.0135
	(0.0180)	(0.0129)	(0.0274)	(0.0415)	(0.0143)	(0.0257)	(0.0409)
Weekly hours	0.109	-0.191	0.434	1.280	0.0826	0.193	-0.0457
	(0.836)	(0.743)	(1.178)	(2.376)	(0.740)	(1.183)	(1.949)
Full-time work	0.0107	0.00339	0.0162	0.0366	0.00991	0.0118	-0.000265
	(0.0220)	(0.0192)	(0.0314)	(0.0625)	(0.0196)	(0.0309)	(0.0488)
Sample: HS or less, excluding non-workers							
Weeks worked	0.612	0.546	0.678	2.576**	0.938**	0.500	1.704
	(0.494)	(0.377)	(0.892)	(1.089)	(0.423)	(0.823)	(1.281)
Full year work	0.0181	0.0221	0.0112	0.0539*	0.0375**	0.00320	0.0282
	(0.0188)	(0.0172)	(0.0269)	(0.0279)	(0.0182)	(0.0240)	(0.0371)
Weekly hours	0.543	0.863*	0.145	0.359	1.013**	0.104	-1.175
	(0.423)	(0.456)	(0.640)	(1.393)	(0.389)	(0.640)	(1.309)
Full-time work	0.0235	0.0295*	0.0130	0.0241	0.0333**	0.0135	-0.0251
	(0.0171)	(0.0154)	(0.0255)	(0.0548)	(0.0147)	(0.0255)	(0.0459)
Observations	5887	2213	3674	1225	2196	3691	1384
Couma-events	743	281	462	156	277	466	177

Note: Estimates for people age 16-70 with high school or less education. All models control for couma by event and year fixed effects, log couma population, log state unemployment rate and log state GDP. Observation weighted by population. * p<0.10 ** p<0.05 *** p<0.01

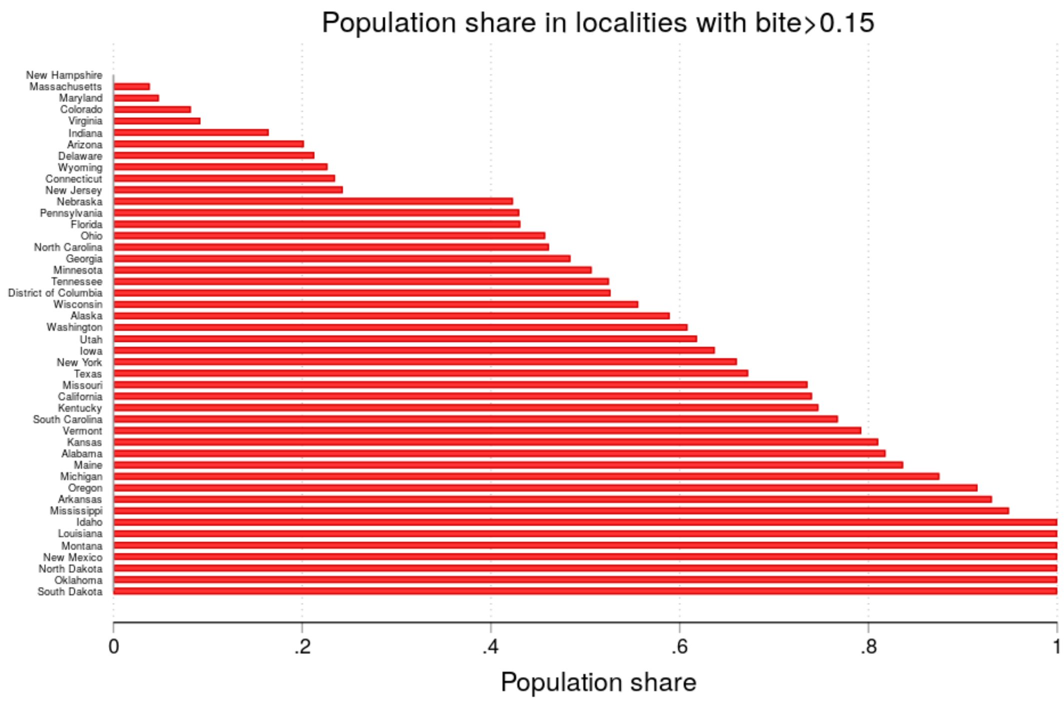
Table 4 Wage and employment effects: county-level administrative data

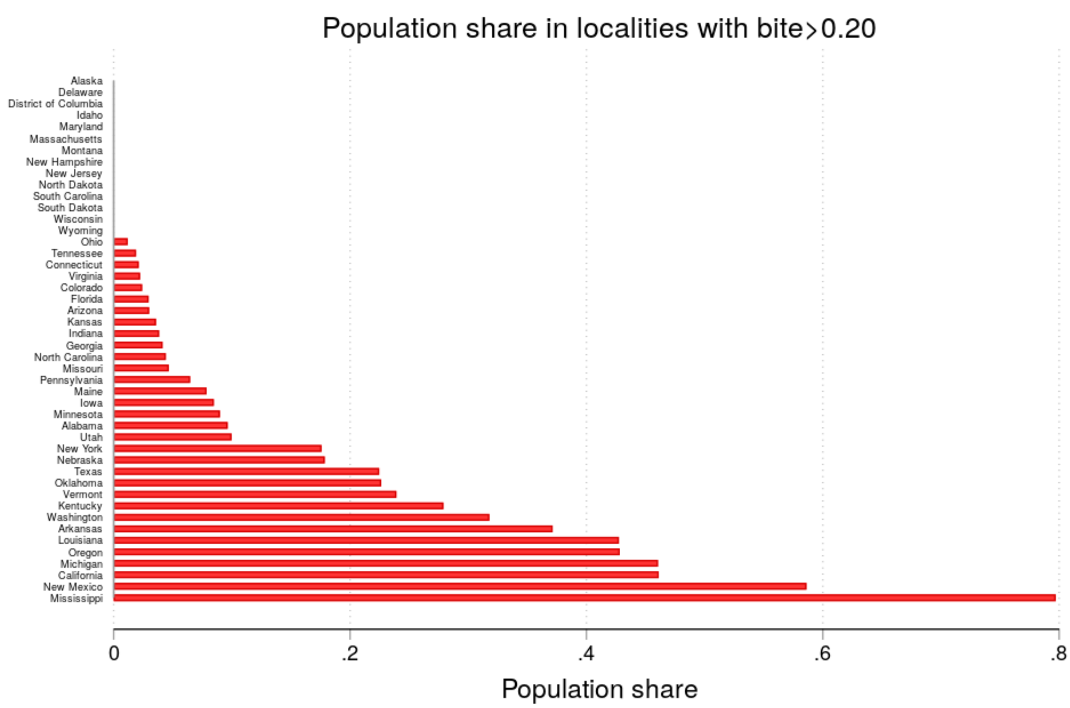
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Localities:	All	KR<50%	KR>50%	KR>60%	LTMW<15%	LTMW>15%	LTMW>20%
Sample: Less than high school							
Log wage	-0.0116	-0.0156	-0.0102	0.0431	-0.0151	-0.00667	-0.00366
	(0.0236)	(0.0257)	(0.0293)	(0.0803)	(0.0238)	(0.0334)	(0.0676)
Employment	0.000431	0.000403	-0.000191	-0.00237	-0.0000878	-0.000240	-0.00179
	(0.00163)	(0.00211)	(0.00263)	(0.00535)	(0.00227)	(0.00270)	(0.00435)
Sample: High school or less							
Log wage	-0.0120	-0.0223	-0.00475	0.0551	-0.0222	-0.0000336	0.0242
	(0.0241)	(0.0276)	(0.0286)	(0.0714)	(0.0287)	(0.0318)	(0.0592)
Employment	-0.0119	-0.00698	-0.0193**	-0.00623	-0.00961	-0.0166**	-0.00381
	(0.00723)	(0.00796)	(0.00869)	(0.0171)	(0.00992)	(0.00785)	(0.0116)
Sample: Food service							
Log wage	0.167***	0.0953***	0.217***	0.239***	0.119***	0.204***	0.223***
	(0.0272)	(0.0307)	(0.0313)	(0.0416)	(0.0311)	(0.0326)	(0.0354)
Employment	-0.000829	-0.00115	-0.000787	0.00245	-0.00216	0.000237	0.00437
	(0.00180)	(0.00242)	(0.00241)	(0.00378)	(0.00248)	(0.00229)	(0.00389)
Sample: Retail							
Log wage	0.0550*	0.00259	0.0927***	0.124**	0.0239	0.0763***	0.110**
	(0.0283)	(0.0346)	(0.0230)	(0.0528)	(0.0362)	(0.0255)	(0.0437)
Employment	0.000978	-0.000856	0.00111	0.00366	-0.00104	0.00139	0.00200
	(0.00172)	(0.00235)	(0.00201)	(0.00399)	(0.00271)	(0.00212)	(0.00270)
Observations	99057	18006	81051	24919	19419	79638	25421
Counties	3109	568	2541	791	620	2494	803

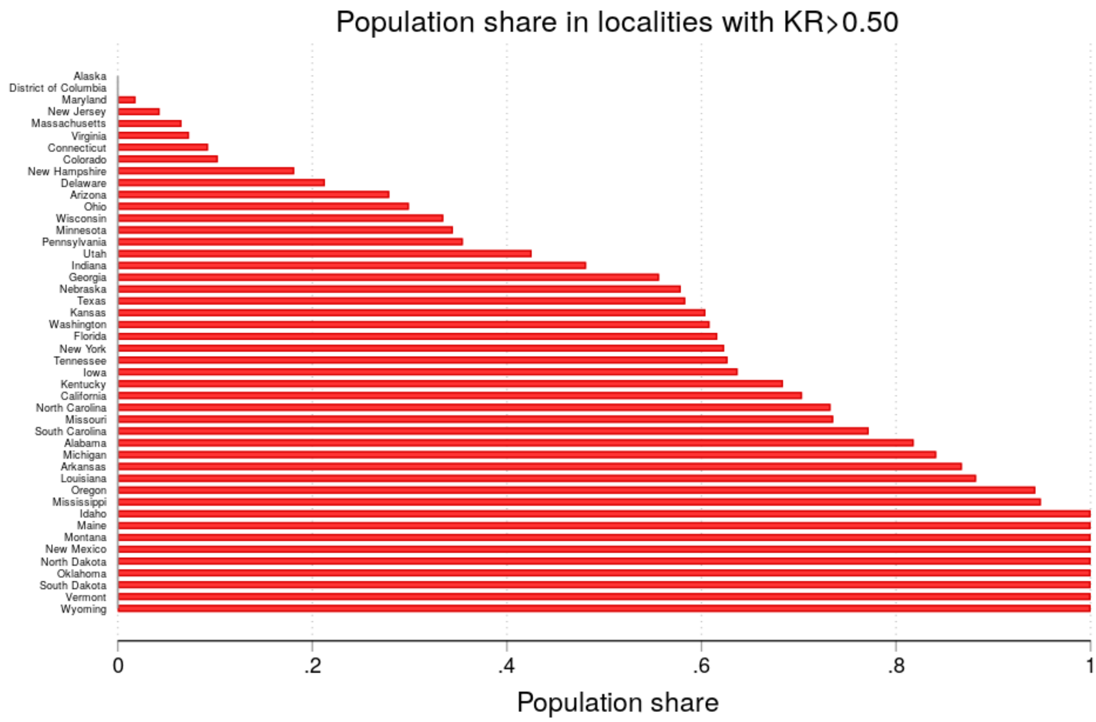
Note: All models control for county by event and year fixed effects, log state population, log state unemployment rate and log state GDP. Data by educational attainment is from the QWI, data by industry is from the QCEW. Observation weighted by population. Standard errors clustered on the state level. * p<0.10 ** p<0.05 *** p<0.01

2. Appendix A Additional exhibits

Figure A1 Population share by Kaitz ratio and bite






Population share in localities with KR>0.50
Alaska
District of Columbia
Maryland
New Jersey
Massachusetts
Virginia
Connecticut
Colorado
New Hampshire
Delaware
Arizona
Ohio
Wisconsin
Minnesota
Pennsylvania
Utah
Indiana
Georgia
Nebraska
Texas
Kansas
Washington
Florida
New York
Tennessee
Iowa
Kentucky
California
North Carolina
Missouri
South Carolina
Alabama
Michigan
Arkansas
Louisiana
Oregon
Mississippi
Idaho
Maine
Montana
New Mexico
North Dakota
Oklahoma
South Dakota
Vermont
Wyoming
0
.2
.4
.6
.8
1
Population share

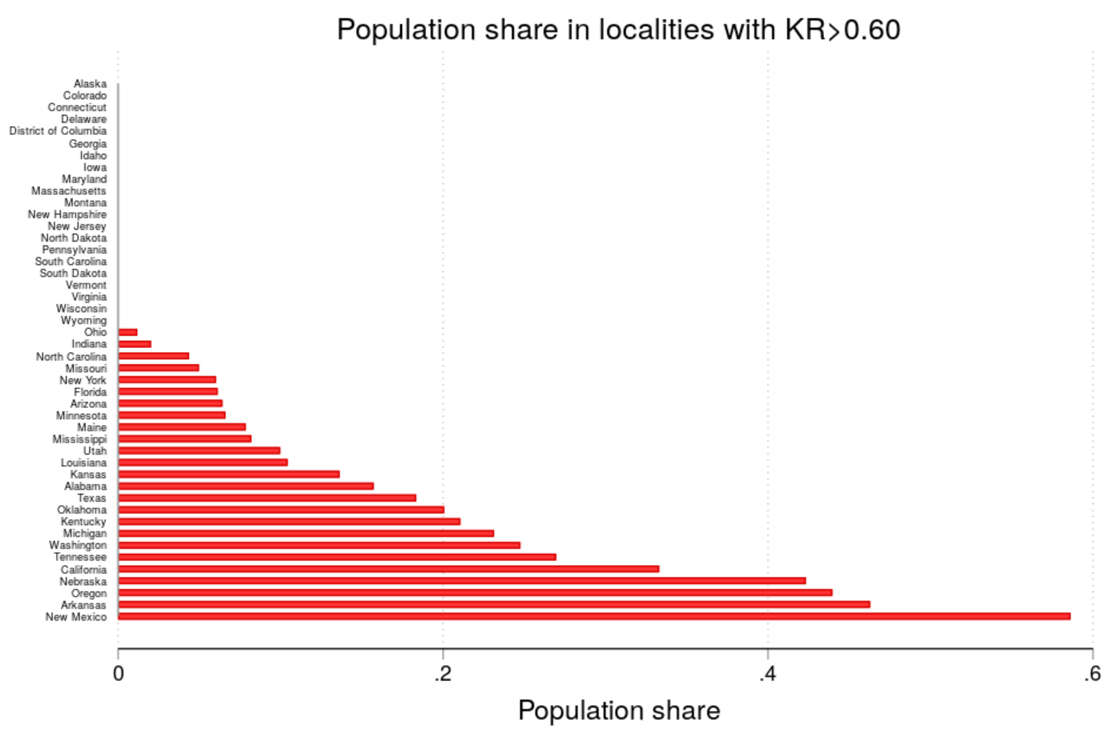
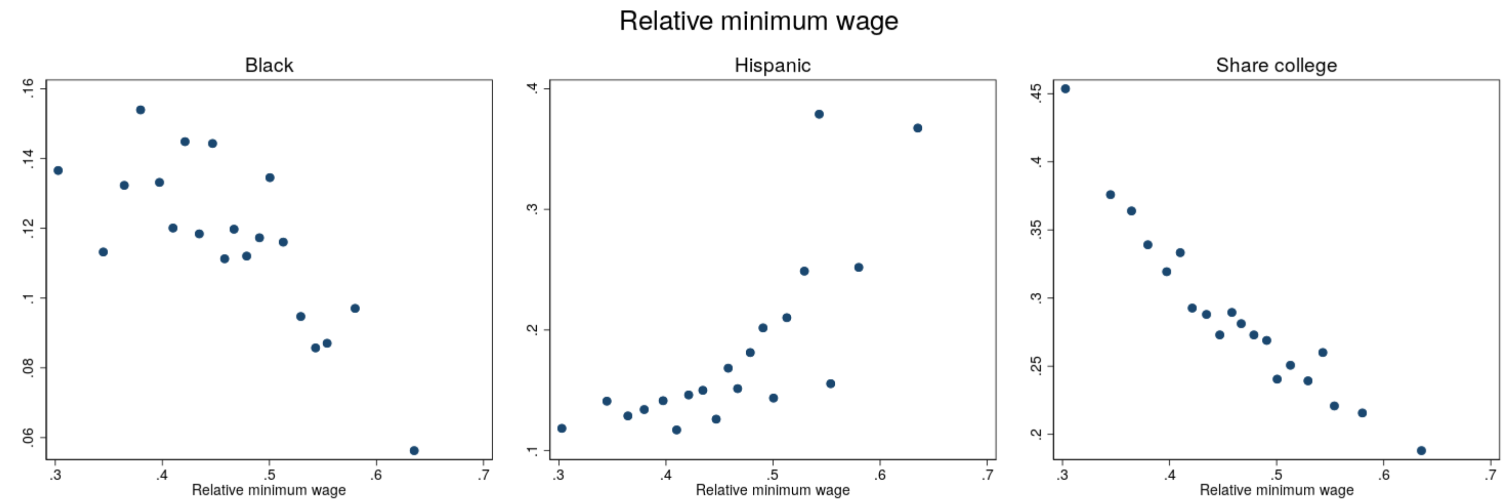
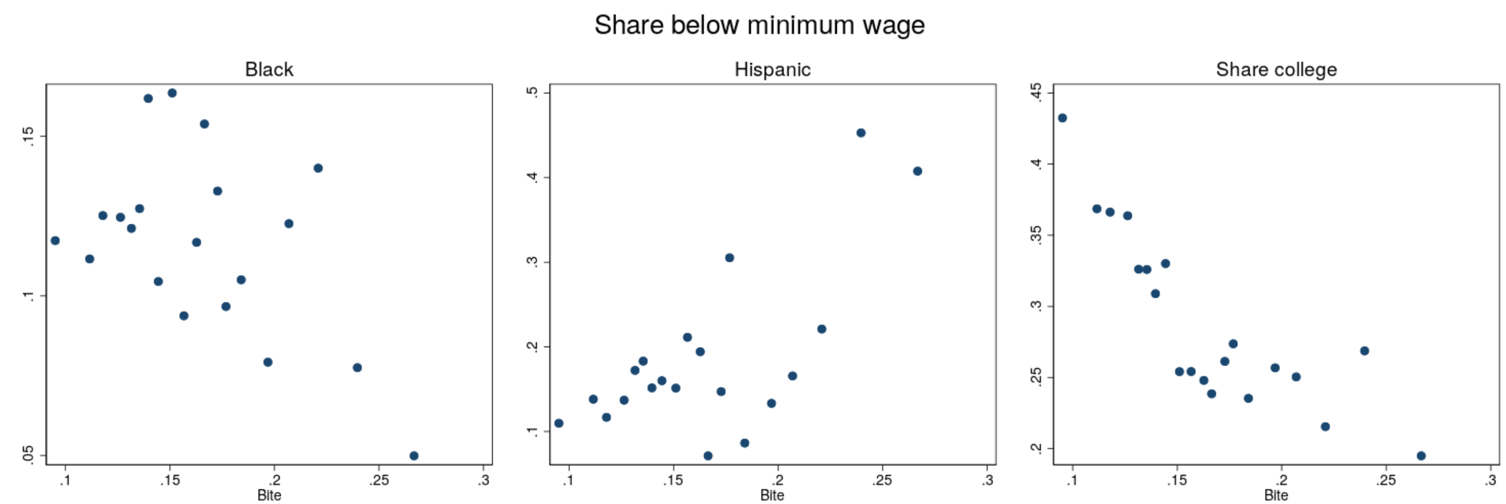

Population share in localities with KR>0.60
Alaska
Colorado
Connecticut
Delaware
District of Columbia
Georgia
Idaho
Iowa
Maryland
Massachusetts
Montana
New Hampshire
New Jersey
North Dakota
Pennsylvania
South Carolina
South Dakota
Vermont
Virginia
Wisconsin
Wyoming
Ohio
Indiana
North Carolina
Missouri
New York
Florida
Arizona
Minnesota
Maine
Mississippi
Utah
Louisiana
Kansas
Alabama
Texas
Oklahoma
Kentucky
Michigan
Washington
Tennessee
California
Nebraska
Oregon
Arkansas
New Mexico
0
.2
.4
.6
Population share

Figure A2 Share black, Hispanic and college graduates, by Kaitz ratio and bite



Source: 2005-2017 ACS



Source: 2005-2017 ACS

Table A1 Minimum wage events

	First event			Second event		
State	Year	First yr (%)	Total (%)	Year	First yr(%)	Total (%)
AK	2010	5%	9%			
AL	2007	10%	32%			
AR	2009	11%	12%			
AZ	2007	27%	32%			
CA	2007	8%	11%	2014	11%	21%
CO	2007	29%	33%			
CT	2009	5%	7%	2014	4%	13%
DC	2008	4%	14%	2014	13%	35%
DE	2007	5%	11%	2014	5%	12%
FL	2009	7%	7%			
GA	2007	10%	32%			
IA	2007	17%	32%			
ID	2007	10%	32%			
IN	2007	10%	32%			
KS	2007	10%	32%			
KY	2007	10%	32%			
LA	2007	10%	32%			
MA	2007	8%	11%			
MD	2007	16%	32%			
ME	2009	4%	9%			
MI	2014	8%	11%			
MN	2009	11%	14%	2014	9%	22%
MO	2007	23%	32%			
MS	2007	10%	32%			
MT	2007	16%	32%			
NC	2007	16%	32%			
ND	2007	10%	32%			
NE	2007	10%	32%			
NH	2007	23%	32%			
NJ	2014	12%	13%			
NM	2007	10%	37%			
NY	2013	9%	20%			
OH	2007	29%	33%			
OK	2007	10%	32%			
OR	2009	6%	6%			
PA	2007	35%	35%			
SC	2007	10%	32%			
SD	2007	10%	32%			
TN	2007	10%	32%			
TX	2007	10%	32%			
UT	2007	10%	32%			
VA	2007	10%	32%			
VT	2009	5%	5%			
WA	2009	6%	6%			
WI	2009	11%	11%			
WY	2007	10%	32%			

Note: table shows minimum wage events included in event study sample.

Table A2 Employment elasticities and elasticities of the poverty rate

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Localities:	All	KR<50%	KR>50%	KR>60%	LTMW<15%	LTMW>15%	LTMW>20%
Sample: Less than high school							
El wrt MW	0.020	-0.038	0.076	0.068	0.013	0.031	0.091
El wrt own wage	0.176	-	0.576	0.390	-	0.196	0.505
Sample: High school or less							
El wrt MW	0.008	-0.018	0.037	0.082	0.002	0.019	0.041
El wrt own wage	0.124	-	0.341	0.456	-	0.222	0.284
Sample: Teens							
El wrt MW	-0.026	-0.114	0.086	0.130	0.013	-0.052	-0.088
El wrt own wage	-0.159	-	0.413	0.400	-	-0.247	-0.383
Poverty - el wrt MW							
Poverty all	-0.029	0.023	-0.086	-0.406	-0.020	-0.065	-0.305
Child poverty	-0.030	0.031	-0.080	-0.470	0.044	-0.106	-0.278
Observations	5887	2213	3674	1225	2196	3691	1384
Coumas	743	281	462	156	277	466	177

Note: Own-wage employment elasticities reported only for subsamples where the wage elasticity of the wage with respect to the minimum wage was significant at the 5 percent level.

Table A3 Wage and employment outcomes, high school or less, by gender and race/ethnicity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Localities:	All	KR<50%	KR>50%	KR>60%	LTMW<15%	LTMW>15%	LTMW>20%
Sample: Women							
Log wage	0.0719***	0.0457*	0.0904***	0.120**	0.0423	0.0907***	0.124**
	(0.0266)	(0.0262)	(0.0321)	(0.0496)	(0.0307)	(0.0302)	(0.0485)
Employment	-0.00365	-0.0188	0.0112	0.0167	-0.0172	0.00944	0.0363
	(0.0190)	(0.0210)	(0.0212)	(0.0274)	(0.0199)	(0.0240)	(0.0243)
Sample: Men							
Log wage	0.0503	-0.0319	0.117***	0.210***	0.00633	0.0766**	0.156*
	(0.0313)	(0.0291)	(0.0372)	(0.0715)	(0.0310)	(0.0351)	(0.0790)
Employment	0.00299	-0.0176	0.0248	0.0498	-0.0104	0.0154	0.0195
	(0.0212)	(0.0181)	(0.0302)	(0.0558)	(0.0182)	(0.0284)	(0.0468)
Sample: Black/Hispanic							
Log wage	0.0676**	0.0730**	0.0432	0.226**	0.0914**	0.0187	0.206**
	(0.0301)	(0.0356)	(0.0462)	(0.0964)	(0.0344)	(0.0427)	(0.0949)
Employment	-0.0425	-0.0414	-0.0447	-0.0462	-0.0537	-0.0386	-0.0229
	(0.0299)	(0.0304)	(0.0441)	(0.0693)	(0.0325)	(0.0376)	(0.0630)
Sample: White non-Hispanic							
Outcome: Log wage	0.0510*	0.0165	0.0778**	-0.0476	0.0282	0.0711*	-0.0667
	(0.0290)	(0.0324)	(0.0368)	(0.0925)	(0.0368)	(0.0365)	(0.0868)
Outcome: Employment	0.00251	-0.0225	0.0270	0.0832**	-0.00996	0.0132	0.0455
	(0.0170)	(0.0156)	(0.0239)	(0.0365)	(0.0172)	(0.0230)	(0.0373)
Observations	5887	2213	3674	1225	2196	3691	1384
Couma-events	743	281	462	156	277	466	177

Note: Estimates for subsamples of people age 16-70 with high school or less. All models control for couma by event and year fixed effects, log couma population, log state unemployment rate and log state GDP. Observation weighted by population.
* p<0.10 ** p<0.05 *** p<0.01

Table A4: Robustness - accounting for differential trends

	(1)	(2)	(3)	(4)	(5)	(6)
Localities:		KR>50%			LTMW>15%	
Specification:	State trend	Covar trend	Covar x yr FE	State trend	Covar trend	Covar x yr FE
Sample: Less than high school						
Log wage	0.168***	0.0983**	0.0987*	0.189***	0.122***	0.103**
	(0.0450)	(0.0401)	(0.0519)	(0.0399)	(0.0355)	(0.0412)
Employment	-0.0105	0.0262	-0.0136	-0.0197	0.0184	-0.0227
	(0.0515)	(0.0380)	(0.0223)	(0.0450)	(0.0394)	(0.0275)
Sample: High school or less						
Log wage	0.130***	0.0733**	0.0699**	0.108***	0.0599**	0.0553*
	(0.0393)	(0.0334)	(0.0303)	(0.0383)	(0.0289)	(0.0297)
Employment	0.0357	0.0225	0.00672	0.0292	0.0189	0.00412
	(0.0270)	(0.0281)	(0.0189)	(0.0260)	(0.0293)	(0.0199)
Sample: Teens						
Log wage	0.242***	0.133**	0.116*	0.230***	0.148**	0.0812
	(0.0674)	(0.0618)	(0.0603)	(0.0614)	(0.0577)	(0.0640)
Employment	0.0608	0.0161	-0.0364	0.0151	-0.0330	-0.0618
	(0.0421)	(0.0373)	(0.0340)	(0.0483)	(0.0417)	(0.0381)
Sample: All						
Poverty	-0.0460**	-0.0518*	-0.0255*	-0.0386*	-0.0487*	-0.0163
	(0.0212)	(0.0267)	(0.0152)	(0.0223)	(0.0254)	(0.0137)
Child poverty	-0.0633*	-0.0785*	-0.0203	-0.0613*	-0.0854**	-0.0247
	(0.0337)	(0.0400)	(0.0263)	(0.0305)	(0.0351)	(0.0242)
Observations	3674	3510	3510	3691	3536	3536
Couma-events	462	441	441	466	446	446

Note: All models control for couma event and year fixed effects, log couma population, log state unemployment rate and log state GDP. Observations weighted by population, adjusting for duplicate observations. * p<0.10 ** p<0.05 *** p<0.01

Appendix B: Relationship to other estimators

In this paper, we use the Kaitz ratio and the bite of the minimum wage to rank coumas by expected impact of minimum wage changes. Our variable of interest in the estimated models is the minimum wage itself, parametrized as its natural logarithm.

In the literature meanwhile, the Kaitz ratio and the minimum wage bite are frequently used as explanatory variables to estimate effects of minimum wages.[26] In this appendix, we discuss these methods in some detail and show how the results from these specifications compare with the findings of our preferred models.

Equation B1 shows the first of the two specifications. The model is similar to our preferred generalized difference-in-differences specification from equation (2). The key difference is that the relative minimum wage—the Kaitz ratio-- is the variable of interest. Note that in these models we cannot replace the couma fixed effects with state fixed effects without changing the results, as within-state Kaitz ratios typically vary significantly at a given minimum wage.

$$y_{ct} = \theta_c + \theta_t + X_{it}\beta + KR_{ct}\gamma^{KR} + \varepsilon_{ct} \qquad \text{(B1)}$$

In model (B1), the parameter of interest γ^{KR} is identified by within-couma variation in the ratio of the minimum to the median wage. As discussed in the paper, this measure will in turn be affected by changes to the median wage that result from local business cycle fluctuations, in addition to changes in minimum wage policies.

The second model follows Card (1992), who uses variation in the bite of the minimum wage to estimate effects on wages and employment. This model is frequently used to evaluate minimum wage changes for which there is no control group, such as a national minimum wage change that is binding for all localities. We have adapted the model somewhat to our setting-- where we have multiple policy changes across states and years. Formally, we model outcomes in couma-event *c* in state-event *s* year *t*:

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[26] For example, Wehby, Dave and Stewart (2018) use the relative minimum wage as their RHS variable; Card (1992) and Bailey, DiNardo and Stewart (2018) use the minimum wage bite.

$$y_{cst} = \theta_c + \theta_s \times post_{st} + \theta_t + X_{it}\beta + (ltmw_c \times post_{st})\gamma^{LTMW} + \varepsilon_{ct} \quad \text{(B2)}$$

where $post_{st}$ is an indicator variable that is equal to 1 in the year of the initial minimum change and later years and $ltmw_c$ is the "bite" of the minimum wage in couma c. As before, the bite is defined as the share of workers whose hourly wage is less than the minimum wage at the end of the four-year event window (to allow for phase-ins).[27]

Here, the parameter of interest γ^{LTMW} is identified from variation in the minimum wage bite across coumas (within each state). That is, the effects of the minimum wage are identified by comparing how outcomes change differentially following policy changes in localities where different shares of the population are expected to receive a wage increase.

We show the results from these specifications in tables B1 and B2. Equation (1) yields negative estimates of γ^{KR} for wages for all three education groups: less than high school, high school or less and bachelor's degree or higher. This finding is consistent with lower median wages correlating with lower wages across the board as well as higher *relative* minimum wages. In other words, the standard criticisms against using the relative minimum wage as a minimum wage metric appear to be validated. Meanwhile, employment effects are positive for less educated workers and negative for the more educated group. Similarly, higher Kaitz ratios are associated with significantly higher levels of poverty. Again, this result likely reflects changes in the denominator of the Kaitz ratio rather than the minimum wage itself.

Table B2 shows results from equation (B2). Overall, these results seem more intuitively plausible. Consistent with what we would expect if the models were correctly specified, these models find significant effects of the minimum wage on wages for less-educated workers, but not for the placebo samples of BA+ workers. Overall, the model fails to find significant employment losses for the affected demographic groups. Meanwhile, a negative effect on employment of more educated workers raises concerns about possible misspecifications.

To summarize, using the relative minimum wage as the RHS variable leads to implausible results, such as negative effects on wages, and is not recommended. These findings

[27] We follow Bailey et al. (2019) and use the share below the new minimum wage rather than the share between the old and the new because of the significant measurement error in hourly wages computed from the ACS (weeks worked is available only in bins).

are consistent with our observation that variation in the relative minimum wage mainly reflects variation in the median wage rather than in the minimum wage. Using the minimum wage bite as the RHS variable generates more plausible results, but some concerns about misspecification remain. Moreover, using the bite as the RHS variable assumes that minimum wage and employment effects are proportional to the bite. Our preferred specifications explicitly allow for wage and employment effects to vary across bites in a nonlinear manner; our results in the main part of this paper show that the wage effects are indeed heterogeneous across bites. It may still be the case that using the bite as a RHS variable is not problematic in some contexts. Nonetheless, our analysis here provides a basis for preferring the specifications that we use in the main part of this paper.

Table B1 Wage and employment effects with the relative minimum wage as the RHS variable

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Localities:	All	KR<50%	KR>50%	KR>60%	LTMW<15%	LTMW>15%	LTMW>20%
Sample: Less than high school							
Log wage	-0.308***	-0.143	-0.495***	-0.737***	-0.172	-0.453***	-0.658***
	(0.0967)	(0.114)	(0.108)	(0.198)	(0.118)	(0.111)	(0.173)
Employment	0.0939***	0.0501	0.121***	0.123***	0.105*	0.0801**	0.131***
	(0.0259)	(0.0478)	(0.0307)	(0.0366)	(0.0527)	(0.0363)	(0.0367)
Sample: High school or less							
Log wage	-0.398***	-0.338***	-0.514***	-0.613***	-0.332***	-0.498***	-0.591***
	(0.0505)	(0.0539)	(0.0593)	(0.117)	(0.0565)	(0.0585)	(0.0868)
Employment	0.0304*	-0.0126	0.0557**	0.0775**	0.0129	0.0374	0.0481*
	(0.0173)	(0.0227)	(0.0219)	(0.0288)	(0.0247)	(0.0240)	(0.0269)
Sample: Teens							
Log wage	-0.0489	0.0773	-0.210***	-0.265**	0.0876	-0.188**	-0.335***
	(0.0786)	(0.118)	(0.0683)	(0.0991)	(0.119)	(0.0702)	(0.0707)
Employment	0.0475	-0.0235	0.0896**	0.0696	0.0597	0.0369	0.0702
	(0.0320)	(0.0501)	(0.0395)	(0.0563)	(0.0592)	(0.0447)	(0.0504)
Sample: BA+							
Log wage	-0.326***	-0.356***	-0.340***	-0.503***	-0.351***	-0.342***	-0.525***
	(0.0492)	(0.0565)	(0.0504)	(0.0791)	(0.0598)	(0.0516)	(0.0583)
Employment	-0.0633***	-0.0526***	-0.0608**	-0.0601	-0.0619***	-0.0555**	-0.0500
	(0.0162)	(0.0181)	(0.0241)	(0.0445)	(0.0177)	(0.0236)	(0.0358)
Sample: all							
Poverty	0.0890***	0.0723***	0.113***	0.131***	0.0833***	0.0989***	0.130***
	(0.0119)	(0.0181)	(0.0181)	(0.0406)	(0.0225)	(0.0148)	(0.0351)
Child poverty	0.143***	0.115***	0.180***	0.198***	0.160***	0.144***	0.222***
	(0.0247)	(0.0401)	(0.0299)	(0.0517)	(0.0386)	(0.0287)	(0.0503)
Observations	5887	2213	3674	1225	2196	3691	1384
Coumas	743	281	462	156	277	466	177

Note: All models control for couma by event and year fixed effects, log state population, state unemployment rate and state GDP per capita. Observations weighted by population. * p<0.10 ** p<0.05 *** p<0.01

Table B2 Wage and employment effects with the share wage<MW as the RHS variable

	(1)	(2)	(3)	(4)
Sample:	LTHS	HS or less	Teens	BA+
Log wage	0.177	0.254***	0.302**	-0.0826
	(0.117)	(0.0693)	(0.136)	(0.0904)
Employment	0.0145	-0.0113	-0.00398	-0.0639**
	(0.0286)	(0.0239)	(0.0464)	(0.0304)
Observations	6394	6394	6394	6394
Coumas	743	743	743	743

Appendix C: Event study models estimated on quarterly data

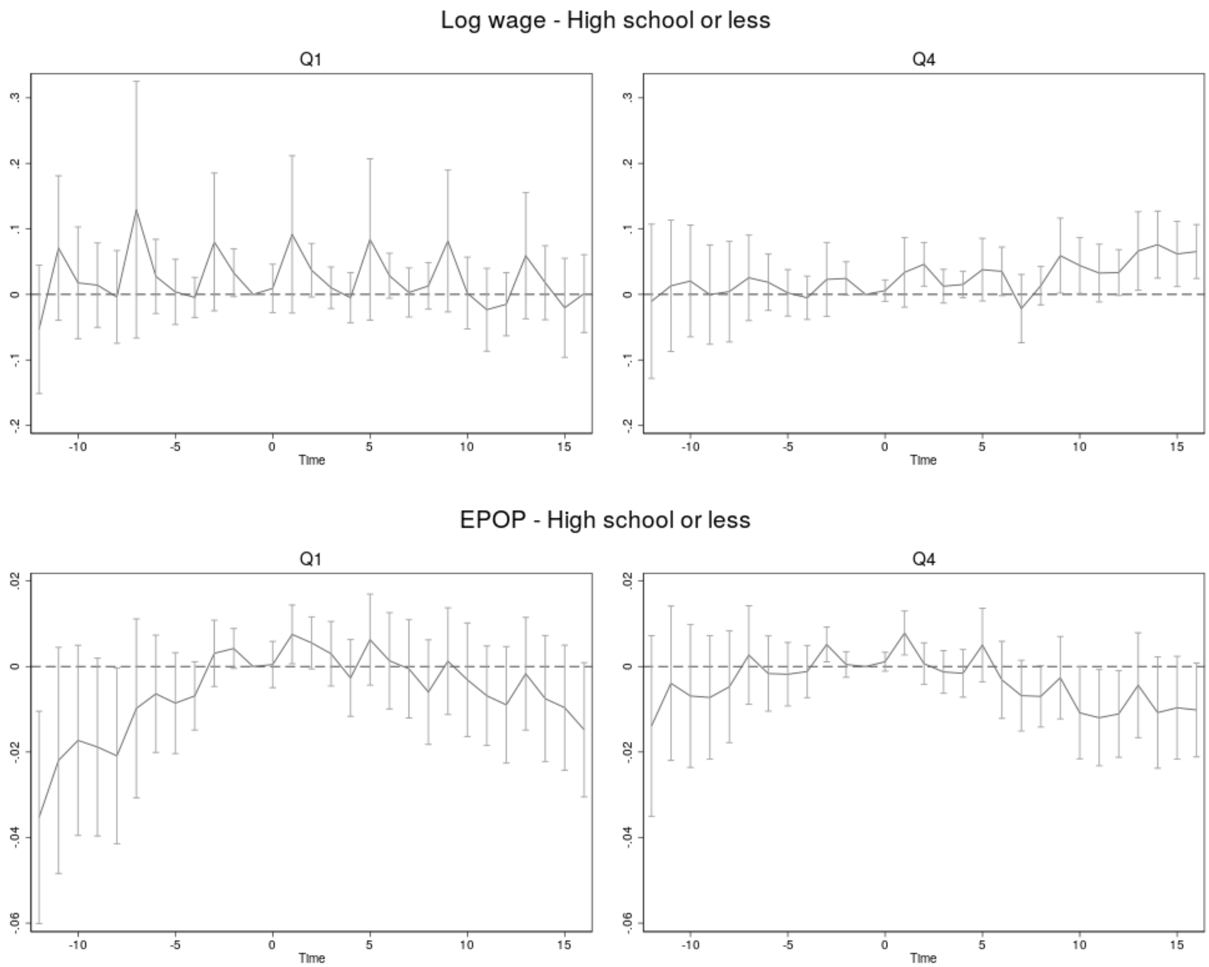
The QWI and the QCEW report data on a quarterly basis. Our preferred event study results using these data sources estimate a set of annualized models. This approach allows for a straightforward comparison with the results from the ACS, as well as sidestepping issues related to seasonal variation in outcomes across locations. Here, we present results from an alternative specification that defines event time in quarters since a minimum wage increase.

Letting *c* and *t* denote couma-event and calendar time (here defined by year and quarter), the regression model can be written as follows:

$$y_{cst} = \theta_{cq} + \theta_t + X_{ct}\beta + \sum_{k=-12,k\neq 1}^{16} \left(\pi_{k(c,t)} \times \delta_c\right)\rho^k + \varepsilon_{ct} \tag{C1}$$

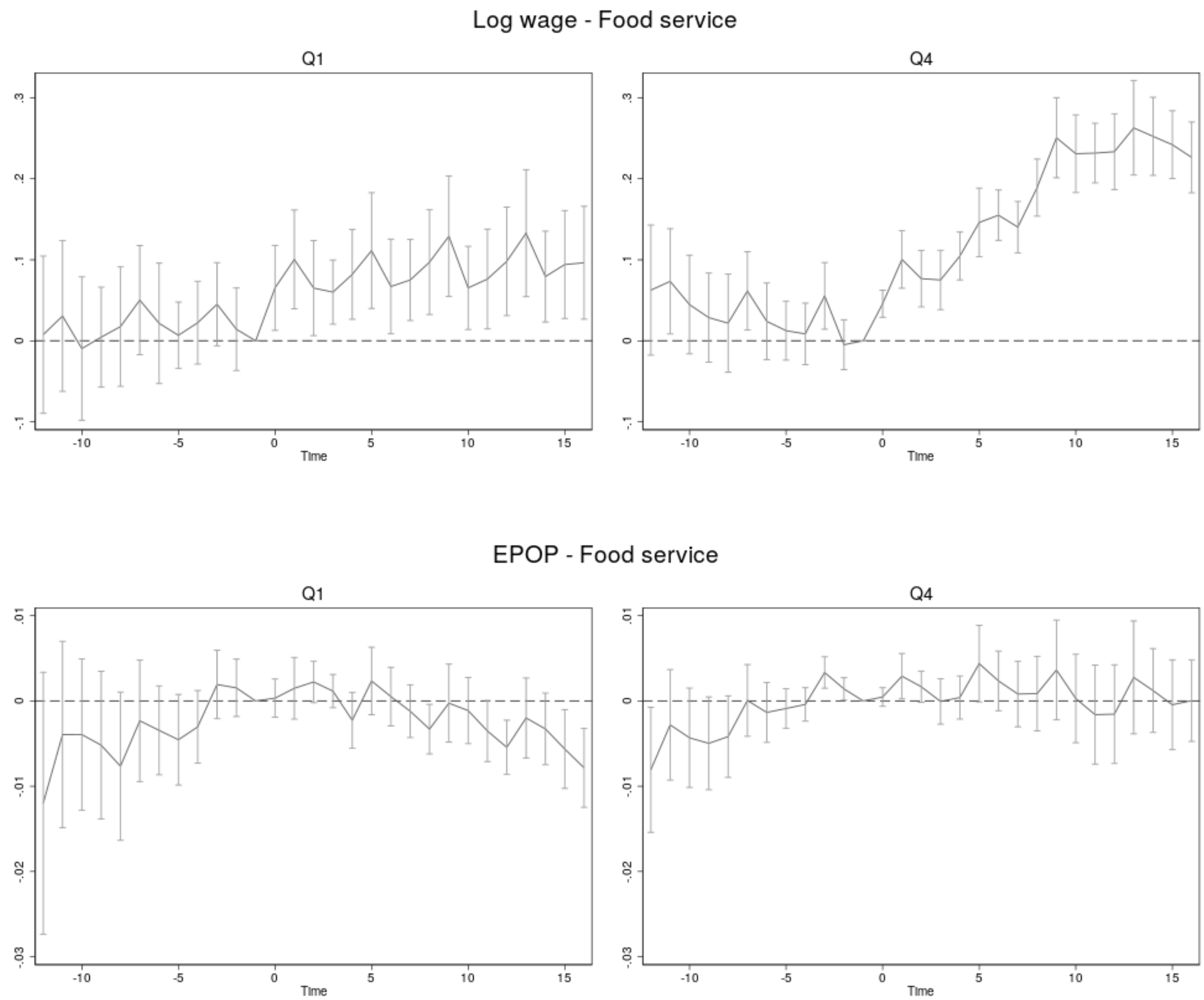
This equation builds on the baseline specifications in equation (1). The models include couma-event and quarterly calendar time fixed effects. To account for differential patterns of seasonal variation in earnings and employments across localities, these models additionally include a set of couma-event specific quarter-of-year dummies. Results from these models are presented in Figures C1-C3. Although these quarterly results exhibit seasonal variation, the overall patterns are similar to the findings in the baseline models presented in Figures 8 and 9.

Figure C1 Wage and employment for non-college workers, by Kaitz ratio quartiles, QWI



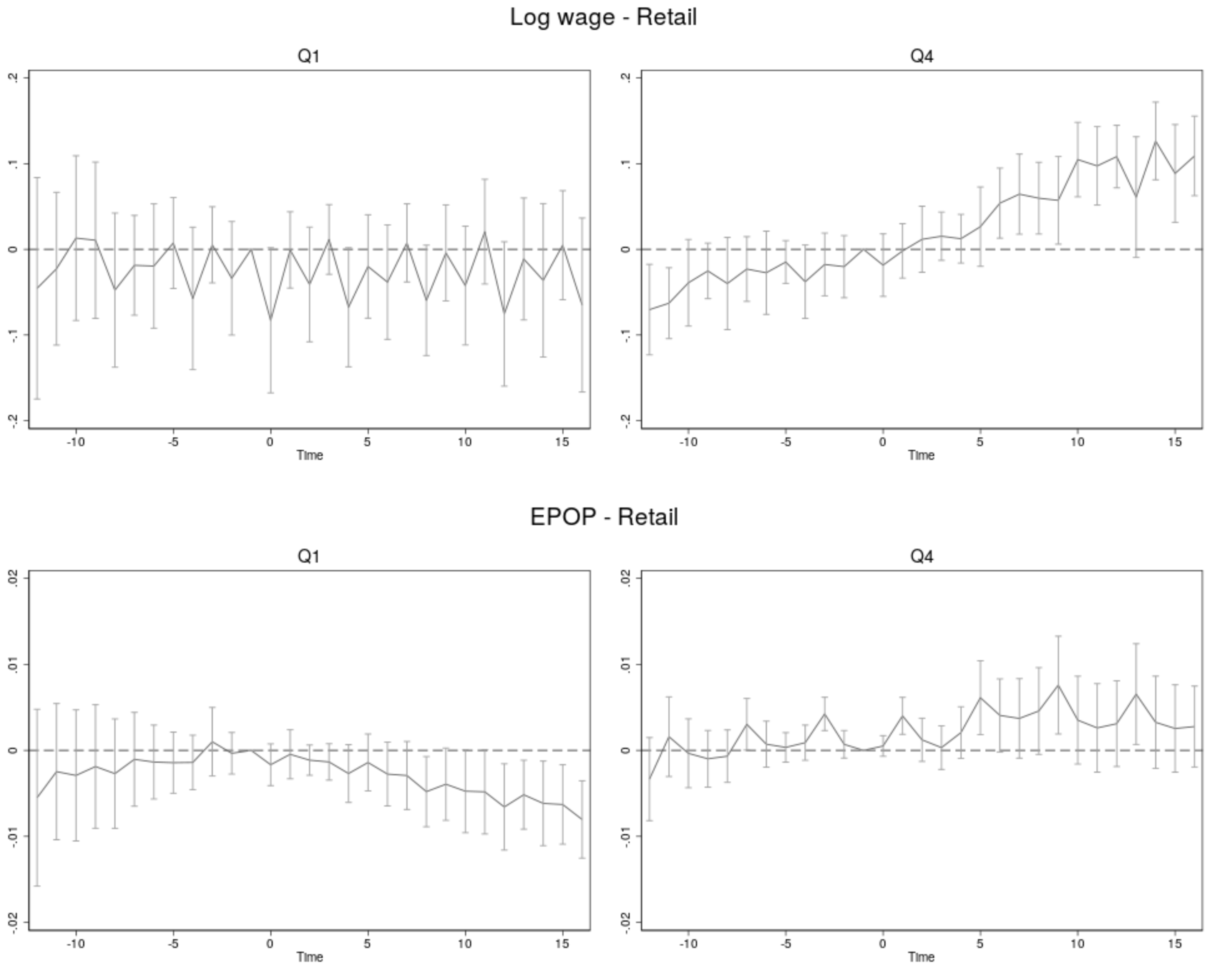
Note: Figure plots estimated event study coefficients from equation (C1) estimated on coumas in the top and bottom quartiles of the relative minimum wage (Kaitz ratio) distribution. The dependent variables are log average weekly wage (upper panel) and employment to population ratio, defined as the ratio of employment of non-college workers to the county level population ages 16-70. Source: QWI.

Figure C2 Results by industry Kaitz ratio quartiles – food service



Note: Figure plots estimated event study coefficients from equation (C1) estimated on coumas in the top and bottom quartiles of the relative minimum wage (Kaitz ratio) distribution. The dependent variables are log average weekly wage (upper panel) and employment to population ratio, defined as the ratio of employment in the food service industry to the county level population ages 16-70. Source: QCEW.

Figure C3 Results by industry Kaitz ratio quartiles – retail



Note: Figure plots estimated event study coefficients from equation (C1) estimated on coumas in the top and bottom quartiles of the relative minimum wage (Kaitz ratio) distribution. The dependent variables are log average weekly wage (upper panel) and employment to population ratio, defined as the ratio of employment in the retail industry to the county level population ages 16-70. Source: QCEW.